

*Samuel Willam M.D. 1776*

PHYSIOLOGICAL  
ESSAYS,

CONTAINING,

- I. An INQUIRY into the Causes which promote the CIRCULATION of the Fluids in the very Small Vessels of Animals.
- II. OBSERVATIONS on the SENSIBILITY and IRRITABILITY of the Parts of Men and other Animals; occasioned by *M. de Haller's* late Treatise on these Subjects.

The SECOND EDITION, corrected and enlarged.

With an APPENDIX, containing an Answer to *M. de Haller's* Remarks in the 4th Volume of the *Memoires sur les parties sensibles et irritables*.

By ROBERT WHYTT, M.D. F.R.S.  
Physician to his MAJESTY.

FELLOW of the ROYAL COLLEGE of PHYSICIANS,  
and PROFESSOR of MEDICINE in the UNIVERSITY  
of EDINBURGH.

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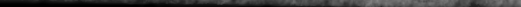

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**I N T O T H E**  
**CAUSES** which promote  
**The CIRCULATION** of the **FLUIDS.**  
**I N T H E**  
*Very SMALL VESSELS. of ANIMALS.*

**The SECOND EDITION** corrected,  
With Additions.

*THE following Paper was read at  
several Meetings of the PHILOSO-  
PHICAL SOCIETY of EDINBURGH,  
in the Years 1745 and 1746; and is  
now published with some Corrections  
and Additions.*



A N  
I N Q U I R Y  
I N T O T H E  
C A U S E S which promote the  
C I R C U L A T I O N of the F L U I D S  
I N T H E  
*Very SMALL VESSELS of Animals.*

**A**LTHO' the Circulation of the Blood has been almost universally acknowledged for above a century past, and much has been written in order to explain this doctrine; yet there are several things relating to it, which have not been, hitherto, accounted for in so satisfactory a manner, as to render any farther inquiry into them altogether superfluous: and of this kind, we presume, is the motion of the fluids in the smaller vessels.

A T L O N D O N J O H N B A R N E S P R I N T E R

THE first authors who embraced the *Harvean* doctrine seem to have ascribed the whole of the circulation, both in the arteries and veins, to the force of the heart\*. But *Borelli*, in whose time it was believed by many, that the arteries and veins were not continued canals, but divided by an intermediate spongy substance, plainly saw, that, in this case, the blood could not be conveyed into the orifices of the nascent veins, by any force of the arterial fluids pushing it forward; and, therefore he supposes it to enter them in the same manner as the particles of water insinuate themselves into a sponge or other porous substance: but as, in his days, the *phaenomena* of capillary tubes were very little known, and the reasons of them not at all understood, 'tis no wonder that, after declaring attraction to be an impossible thing, he ascribes the above effects to the gravity of the fluid itself;† nor does he seem to have been sufficiently aware,

\* Jo. Walaei epist. ad Bartholin. De motu chyli et sanguinis.

† Borelli, De mot. animal. pars 2. prop. 32.

aware, that, after water has risen to a determinate height in small tubes, or a certain quantity of it has been received into porous bodies, no more of it will enter into either of these.

Dr. Pitcairn, in his *Dissertatio de Circulatione sanguinis per vasa minima*, after shewing that animal secretion cannot be performed by means of ferments in the glands, or by these bodies acting as filtres, endeavours to prove, that the various secretions from the blood are intirely owing to the different diameters of the secretory vessels: but he makes it no part of his inquiry, by what powers the fluids are pushed through these vessels. However, that there might appear no difficulty in the motion of the fluids through, even, the smallest tubes of the body, nor any suspicion of their stagnating in them, we have lately been told, that the blood moves more quickly in the smaller than in the larger vessels; an assertion so inconsistent with the laws of hydraulics, when applied to the animal frame, that it could scarcely have been expected to have dropt

from the pen of a writer much less noted than *Dr Hoffman*\*.

BUT, how easy soever it may have appeared to some authors to account for the motion of the fluids in the small vessels of animals, yet whoever impartially considers the resistance that a fluid, moving through the *aorta* and all its branches, must meet with from friction, which increases as the diameters of the vessels decrease, and adds to this the mutual attraction and cohesion between the particles of the fluids and the sides of the vessels in which they move, will not only see that there is, at least, some difficulty in this matter, but be also apt to suspect that neither the force of the heart, nor the alternate contraction of the larger arteries, is sufficient to drive the fluids through the smallest vessels of the brain, *testes*, and many other parts of the body.

IN order, however, to set this affair in a clearer light, we shall particularly consider the several causes to which the circulation of the blood has been commonly ascribed.

S E C T.

\* *Frederic. Hoff. syst. med. l. 1. §. 1. c. vi. No. XVII.*

## S E C T. I.

*Of the force of the heart, contraction of the arteries, gravity, and the attraction of capillary tubes, considered as causes of the circulation of the fluids in the small vessels of animals.*

THE principal cause which propells the blood through the body, is, without doubt, the contraction of the heart: let us then, first, inquire how far this may be supposed sufficient to account for the motion of the fluids in the very small vessels of animals.

IF the force with which the blood is thrown, by the left ventricle of the heart, into the *aorta*, be supposed equal to the pressure of a column of blood 90 inches high\*; the *momentum* of this fluid in any artery will be found, by multiplying the *area* of the transverse section of that artery into 90,

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\* Dr Hales, from a variety of experiments made on horses, dogs, sheep, and other animals, thinks it probable, that the blood would rise seven feet and an half, or 90 inches in a tube fixed into the carotid artery of a middle sized man. Statical Essays, vol. 2. p. 40.



the height of that column of blood, whose pressure is supposed equal to the protrusive force of the heart : for the product gives the number of cubic inches or parts of a cubic inch of blood, whose weight is equal to the pressing power with which the blood is driven by the force of the heart into that artery,

THE diameter of a circulating red globule of blood, has been generally reckoned something less than  $\frac{1}{3000}$  part of an inch ; but Dr Martine has, from *Lewenboeck's* and *Jurin's* later observations, shewn it to be  $\frac{1}{1933.5}$  part of an inch \* ; and *Lewenboeck* has observed, that one of these globules is sometimes obliged, in passing through a very small capillary artery, to change its figure into an oblong spheroid, so that the diameter of such an artery may be supposed nearly equal to that of a red globule. If then, for the sake of more easy computation, we suppose the diameter of a red capillary artery to be equal to  $\frac{1}{2000}$  part of an inch, the area of its transverse section will be

\* *Edinburgh Medical Essays*, vol. 2. art. vii.

0.000 000 196; and this multiplied by 90 gives 0.000 0176 parts of a cubic inch of blood, which amounts to 0.00466 or  $\frac{3}{114}$  part of a grain;\* and is equal to the moment of the blood, arising from the pressing force of the heart, in a capillary artery, whose diameter is  $\frac{1}{2000}$  part of an inch, upon the supposition that there were no loss of motion from friction, and that the areas of the transverse sections of all the capillary arteries in the human body were equal to that of the *aorta*: but since this is not the case, and the areas of the former greatly exceed that of the latter; the moment of the blood in a capillary red artery, will fall very much short of our computation.

To illustrate this; let us suppose a pipe A of an inch diameter, to be divided into several branches, and at last to terminate in 10000

\* A cubic inch of warm blood is reckoned by some 266, and by others a little more than 267 grains: but Dr *Martine* seems to have fixed it pretty accurately at 264 $\frac{1}{4}$ ; and, for the sake of even numbers, I have supposed it to be 265 grains.

10000 small tubes  $a, a, a, a, \&c.$  each  $\frac{1}{100}$  part of an inch in diameter ; the sum of the areas of whose transverse sections is equal to that of A. If a fluid be pushed through such a system of vessels, with any given force, the velocities in the small tubes  $a, a, a, a, \&c.$  will be equal to the velocity in A ; and their *momenta*  $m, m, m, m, \&c.$  all taken together, will, bating friction, be just equal to the *momentum* M in the large trunk A, *i. e.*  $m \ 10000 = M$  or  $m = \frac{M}{10000}$ . But if another pipe B of the same diameter with A be divided so as to terminate in 300000 small tubes  $b, b, b, b, \&c.$  each  $\frac{1}{100}$  part of an inch diameter ; then although a fluid be pushed through the two trunks A and B with the same velocity, and consequently the *momentum* in them be equal, yet the velocity in any one of the small tubes  $a, a, a, \&c.$  will be to the velocity in any one of the corresponding tubes  $b, b, b, \&c.$  as 30 to 1, and consequently their *momenta* will be as 900 to 1.

DR KEILL, having, by measuring the arteries of the human body, fixed the proportions

tions of the branches to their trunks after every division, lays down a method for calculating in what degree the velocity of the blood in the different arteries is affected by the increase of the capacity of the vessels through which it flows: \* according to this computation, it will be found, that the velocity of the blood in an artery whose diameter is  $\frac{1}{1000}$  part of an inch, ought to be to its velocity in the *aorta*, as 1 to 345, and consequently the moment of the blood in such an artery must be  $345 \times 345 = 119025$  times less than we have computed it above, *i. e.*,  $= \frac{1}{114} \times 119024 = \frac{1}{11471150}$  part of a grain. And since a globule of red blood weighs nearly  $\frac{1}{10000000}$  part of a grain †, it follows, that the moment or pressing force of such a globule in its capillary artery, arising from the impulsion of the heart, does not exceed twice its own weight.

BUT even this moment, however small it may appear, must be diminished by friction: the

\* Keill's Tentamen. med. phys. 2.

† Edinburgh Medical Essays, vol. 2. art. vii. § xi.



the precise quantity of which, although it may perhaps, be difficult, with any certainty, to determine; yet that it must be very considerable, will evidently appear from what follows.

1. IF two pipes of equal lengths, whose diameters are  $\frac{3}{1000}$  and  $\frac{2}{1000}$  parts of an inch, be, one after another, screwed into the side of a vessel at the perpendicular distance of four feet from the top of the water, and laid parallel to the horizon; the large pipe will discharge 179, and the small pipe  $6\frac{1}{3}$  ounces of water, in half a minute. Hence the velocities of the water in these two pipes must have been as 1293 and 756; and, were it not for the inequality of the resistance of the air, the velocity in the large pipe would have been still greater, and the velocities in the two pipes pretty nearly as the square-roots of their respective diameters\*.

HENCE, if we could suppose a capillary artery, of  $\frac{1}{2000}$  part of an inch diameter, to go off directly from the beginning of the

\* Robinson's animal oeconomy, prop. 1. exp. 2.



the *aorta*, without any intermediate branchings; the velocity of the blood in it would be (*ceteris paribus*) to the velocity of the blood in the *aorta*, nearly as  $\sqrt{\frac{1}{0.0003}}$  the diameter of the capillary is to  $\sqrt{\frac{1}{0.7}}$  the diameter of the *aorta*, i. e. as 1 to 37.4; and consequently the moment of a single globe in such a capillary artery, would be to its moment in the *aorta*, as 1 to 1398.

2. BUT further, the loss of motion from friction depends not only upon the smallness of the vessels, but also upon their distance from the heart: for, if two cylindrical pipes, whose common diameter is  $\frac{3.45}{1000}$  parts of an inch, and whose lengths are 2 and 8 feet, be screwed into the side of a vessel full of water, at the distance of four feet from the top; the quantities discharged in half a minute, will be  $97\frac{1}{2}$  ounces by the long pipe, and 175 ounces by the short one. Hence the velocities of the water in the two pipes were as  $97\frac{1}{2}$  and 175; so that, by\* the greater quantity of friction in the  
longest

longest pipe, the water lost above  $\frac{2}{5}$  of its velocity\*.

3. AGAIN, the velocity of the blood will be different according to the different angles at which the branches go off from their trunks; and the various flexures and convolutions of the small arterial ramifications must increase the friction in them, and consequently retard the motion of the blood considerably. This seems to be confirmed by an experiment of Dr *Hales*; from which it appears, that the velocity of the blood in the small arteries decreases in a greater proportion than it ought to do, by the above mentioned experiments made with streight cylindrical pipes; for, having slit up the intestines of a dog from one end to the other, on the side opposite to that where the blood vessels enter them, and fixed a brass tube into the descending *aorta*, he found that, with a pressure equal to the force of the heart, only  $\frac{1}{3}$  of the water passed in a given time through the slit arteries of the

\* Robinson's animal oeconomy, prop. 1. exp. 1.

the guts that flowed through the mesenterics when cut over just at their entry into the intestines; notwithstanding that the area of the orifices of all the former exceeded that of the latter, and that the diameters of the cut mesenterics did not exceed four times the diameters of the converging slit arteries of the guts.\*

FROM what has been said it may appear, that the velocity of the blood will not be the same in all the arteries of the same diameter, (as some have fondly imagined, and been at no small pains to prove), but will be greater or less, according to their distance from the heart, the excess of the areas of the branches above their trunks, the angles at which they go off, and the number and degree of their flexures.

AGREEABLE to this, Dr *Hales* observed, that, in a capillary artery of the lungs of a frog (where the distance from the heart is but small, and where the excess of the area of all the branches above their trunk, is not near so great as in the other parts of

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the

\* *Hales's Statical Essays*, vol. 2. exp. ix.

the body), the blood moved forty three times faster than in a capillary artery of one of the muscles of the *abdomen*\*: and it is probable that, next to the lungs, the blood moves quickest thro' the vessels of the heart. In consequence of this quick circulation, it must be evident, whether we suppose animal heat to arise from the friction of the blood on the sides of the vessels, or from an intestine motion among its small particles, that, *ceteris paribus*, more heat must be generated in the lungs and heart than any where else; and hence the necessity of continual supplies of fresh air to cool the blood in its passage through the pulmonary vessels. Nor is this opinion founded in theory alone; for, upon trial, it will appear, that the greatest heat in any animal is, almost always, about the heart. In a jackdaw, the heat below the wing made the mercury in my thermometer rise to 104 degrees of *Farenheit's* scale; within the *intestinum rectum*, it rose to 107½; and, when applied to the heart, it reached 109. And, agreeably

\* Statical Essays, vol. 2. p. 68.

agreeably to this, I have found the heat in a pigeon's heart above a degree greater than within the *intestinum rectum*.

UPON the whole, if the moment of a single red globule of blood arising from the pressing force of the heart, does not in its capillary artery, even bating friction, exceed twice its own weight or  $\frac{1}{25471350}$  part of a grain; and if that loss of motion which it must have sustained by friction in its way from the heart thither be considerable, as one may reasonably conclude from what has been advanced upon this head; it will follow, that the real remaining force of such a globule, when it arrives at a red capillary artery, may probably fall short of its own weight, and must be so extremely small, that it can scarcely be supposed sufficient to overcome the resistance it must meet with, in passing through a vessel by which it is closely embraced on all sides, although the anterior fluid in the capillary veins were no obstacle in its way.



[ I desire it may be here understood that the above calculations are by no means intended as demonstrations, but rather as illustrations, in the present argument concerning the force of the blood in the smaller vessels; and, allowing that by them the moment of a red globule in its capillary artery comes out too small, either from our having, with Dr *Hales*, rated the force of the left ventricle of the heart too low, or, with Dr *Keil*, the number of branchings of the arteries, and the proportion they bear to their trunks, too high; yet it must be evident, that the force of the heart must be insufficient to push the fluids thro' all the inferior orders of vessels, or, which is the same thing, that the left ventricle of the heart does not, by its direct projectile force at every contraction, push on and move forward the whole circulating fluids in all the vessels of the body.

Dr *Hales* observed the blood's motion to be accelerated by every *systole* of the heart, not only in the small arteries, but also in the nascent capillary veins of the lungs of a frog;

frog \*; and *Lewenboeck* assures us he has seen the same thing in other parts of various animals: so that it is not to be doubted, that the projectile force of the heart reaches at least as far as the capillary arteries of the first order, nay, is probably continued, for some small way, along their corresponding veins; especially when these are not far from the heart.

But that the moment of the blood in the red capillary arteries, at any considerable distance from the heart, must be very small, will appear from an observation of *Dr Hales*; according to which the velocity of the blood in one of these arteries in the *abdomen* of a frog, was near 900 times less than the equable velocity of this fluid in the *aorta* of a man†; and consequently 2.6 times less than we have computed it to be in a human red capillary: wherefore the excess of the moment of a red globule, in such an artery of a frog, above the resistance it had to overcome, only amounted to  $\frac{1}{173140000}$  part

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of

\* *Statical Essays*, vol. 2. p. 69.

† *Statical Essays*, p. 47. and 68.

of a grain, and so must have fallen a good deal short of  $\frac{1}{3}$  of its own weight; supposing the globules of red blood in a man and a frog to be of the same magnitude, which does not seem improbable\*.

IF then the remaining moment of a red globule in its capillary artery, after having overcome the resistance of the anterior blood in its corresponding vein, does not amount to  $\frac{1}{3}$  of its own weight; it must be evident, that the serous and smaller globules, which move along with the red ones, must be applied, by the projectile force of the heart, to the orifices of the lateral serous arteries with a very inconsiderable force: such a one, surely, as will be far from being able to push these fluids through the serous, lymphatic, and, for any thing we know, many more inferior orders of vessels.

BUT, to set this matter in a still stronger light, we shall, upon the principles above laid down, endeavour to investigate the force of the heart at the origin of the nerves.

*Lewenhoeck*

\* Med. Essays, vol. 2. art. vii. §. v.

*Lewenboeck* tells us, that he discovered vessels in the cortical part of the brain, which could not admit a globule whose diameter was  $\frac{1}{28000}$  part of an inch;\* and he observed the fibres of its medullary substance to be either quadrangular or hexangular: whence he concludes, that they must be composed of smaller fibres, whose extreme minuteness made it impossible for him to discover any thing of their figure, nor does he think they can ever be seen distinctly by human eyes†.

*Dr Porterfield* has, indeed, from an experiment of *Dr Hook*, computed the diameter of a single nervous fibre to be  $\frac{1}{21000}$  part of an inch‡: but, as the best microscopes have never been able to discover any cavities in the nerves, 'tis certain, that, if they are hollow tubes at all, the diameter of their cavities must be a great deal less than this, and perhaps fall short of  $\frac{1}{200000}$  part of an inch; for a microscope, which magnifies the diameter of an object 800 times, would,

\* *De cerebro*, p. 35.

† *Epist.* 34.

‡ *Edin. Med. Ess.* vol. iv.



would, upon this supposition make the cavities of the nerves appear equal to a point whose diameter is  $\frac{1}{250}$  part of an inch, which is an object that may be discovered by a good eye. *Lewenboeck*, 'tis true, towards the end of his days, and when turned of eighty years, pretended oftner than once to have seen cavities in the nerves very distinctly. But it happens unluckily for this discovery, that no body has been able to confirm it since his death: nor could he, when alive, though he saw these cavities himself, shew them to any one else; as appears from the following passage in his 32d epistle: *Id unum in hoc negotio male me habet, quod cavitates illas nemini possum conspicuas exhibere; nam simulac illas oculis meis examinandas admoveo, ilico et minuto citius per exsiccationem confidunt.* But if the ultimate fibres of the *medulla oblongata* were so fine that he could discover nothing of their shape or figure, as he himself confesses, it will not be thought probable that he could discover the cavities of the nerves, which seem



to be a production of these, and at least equally subtile with them.

BUT, lest any one unaccustomed to speculations of this kind should think the motion of a fluid through such vastly subtile vessels as the nerves almost impossible, let him reflect a little on the infinite divisibility of matter, and particularly on the extreme ductility of gold, which may be drawn over silver so as the thickness of the skin of gold (in which however the best microscope cannot discover the smallest pore) shall not amount to  $\frac{1}{1000000}$  part of an inch<sup>\*</sup>; i. e.  $\frac{1}{60}$  part of what we suppose the diameter of the cavity of a nerve may be: so that the particles of such a leaf of gold swimming in a fluid might pass more easily through the nerves, than a single globule of red blood does through its capillary artery.

FURTHER, a soap-bubble, when managed after Sir *Isaac Newton's* method, exhibits, just before it breaks, a black spot upon its superior part; the thickness of which, according to his theory of light and colours, scarcely

\* Memoires de l'Acad. des Sciences, an. 1713.

scarcely exceeds  $\frac{1}{3000000}$  part of an inch. Hence we see, that a fluid composed of soap and water, may be divided, by human art, into parts whose diameter is fifteen times less than that which we have assigned to the nerves, and consequently that such a compound fluid might easily pass through their cavities.

LET us then suppose the diameter of the cavity of a nerve to amount to  $\frac{1}{300000}$  part of an inch, and the area of its transverse section will be 0.0000000000196; which multiplied into 90 (the height of a column of blood whose weight is supposed equal to the pressing force of the left ventricle of the heart) gives 0.00000000176 parts of a cubic inch of blood, or  $\frac{1}{1240000}$  part of a grain; which would be equal to the moment of the animal spirits at the origin of the nerves, arising from the impulsive force of the heart, if there were no loss of motion from friction, and if the area of the transverse section of the *aorta* were equal to the area of the transverse sections of all the extreme capillary vessels, in which the numerous branches

ches and ramifications derived from the *aorta*, at last terminate. But, if we consider how greatly the latter must exceed the former, and, upon Dr *Keill's* principles, enter into a computation of the effect which this must have upon the motion of the nervous fluid; we shall find, that its velocity will be to that of the blood in the *aorta* nearly as 1 to 20000; and consequently the moment of the nervous fluid, arising from the protrusive force of the heart, will be only equal to  $\frac{1}{2140000} \times \frac{1}{40000000} = \frac{1}{8560000000000}$  of a grain.

If we imagine a sphere to be composed of the particles of the nervous fluid, whose diameter is equal to the diameter which we have assigned to the cavity of a nerve; then, taking its specific gravity to be the same with that of water, its weight will amount to  $\frac{1}{45228780825614}$  part of a grain, *i. e.* near 19 times more than the force with which it is pushed forward by the contraction of the left ventricle of the heart, even upon the supposition that it had met with no resistance from friction in its passage through the

the small vessels of the brain. Hence the *momentum* of a small sphere of animal spirits in a nerve, is 38 times less in proportion to its weight, than the moving force of a globe of red blood in its capillary artery. And the difference of their forces will be still greater, in proportion to the resistance which each has to overcome; since the resistance to the motion of a fluid, from friction, must be, *cæteris paribus*, as much greater in the nerves than in the red capillary arteries, as the diameter of the latter exceeds the diameter of the former.

BUT further, since, the longer any capillary is, the more will the motion of a fluid be retarded, and consequently its force be diminished in it; 'tis easy to see that in the nerves, whose cavities are so inconceivably small, but whose length is generally very considerable, the force of the heart, which we have shewn to be surprisingly little, must be altogether unable to overcome the friction, nay even the mutual attraction of cohesion betwixt them and their fluid, and, consequently, be of itself, and when unassisted by



by any other power, wholly insufficient to propell the animal spirits to all the different parts of the body. And this, even upon the supposition that the nerves were continued directly from the extremely minute capillary arteries: but, if we consider how much the force of the blood must be broken in passing through the infinitely convoluted and amazingly fine vessels of the cortical part of the brain, together with the follicles in which these are imagined, by some, to terminate; what we have been contending for, will appear still more evident.

LASTLY, the above reasoning receives additional weight from those experiments which shew that the brain may be nourished, perform its office, and afford sufficient supply of spirits for carrying on all the vital and animal functions, although the blood is pushed by the heart into its vessels with a great deal less force than usual. Thus the illustrious *Baron Van Swieten* informs us, that he tied both the carotid arteries of a dog without any observable harm to him; on

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the contrary, he continued twelve days  
healthful and lively: after which time he  
opened his skull, but could discover nothing  
praeternatural in the brain\*. Now, as in  
this dog the brain could only be supplied by  
the vertebral arteries which inosculate with  
the carotids, the velocity, and consequently  
the moment of the blood, must, at the same  
time that it was considerably lessened in the  
ramifications of the former, have been  
so remarkably diminished in those of the  
latter, by reason of the smallness of the  
branches with which they communicate,  
compared with the trunks of the carotids, as  
to shew, beyond doubt, that the secre-  
tion of the nervous fluid, and its deri-  
vation to the several parts of the body,  
do not depend so much upon the force of the  
heart, as has been generally imagined, but  
must be, in a great measure, owing to some  
other cause.

HAVING shewn how inconsiderable the  
moment of the fluids arising from the pro-  
jectile force of the heart must be, in the  
inferior

\*Comment. in Boerh. aphor. vol. i. p. 266.

inferior orders of vessels, and<sup>2</sup> particularly at the origin of the nerves; we come now to take a view, somewhat different, of the matter, and to compare the real force of the left ventricle of the heart with the obstacles it has to overcome, upon the supposition that at each *systole* it pushes forward the whole circulating fluids in all the arteries and veins of the body.

*Borelli* computed the resistance which the blood meets with in circulating through all vessels of the human body, to be equal to 180000 pounds weight \*: but though this be over-rating the matter very much, yet, after all the abatements that can be reasonably allowed, there will remain a resistance by much too great to be overcome by the force alone of the left ventricle of the heart; a force, which cannot, in man, amount to above 60 pounds weight †, as far as can be gathered from the latest and best experiments, which have been made on other

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animals

\* De motu animal. part. 2. prop 73.

† De Hales makes it only 51 pounds, *Statical Essays*, vol 2. p. 40.

animals, in order to determine the pressing power of their heart. Yet, inconsiderable as this force is, it is not to be regarded as that which is communicated to the blood in the *aorta*, but only as the pressure or weight sustained by the whole internal surface of the left ventricle of the heart just when it begins to contract; and the force with which the blood is impelled into the *aorta*, will (since fluids press equally *undequaque*) bear no greater proportion to this, than the *area* of the orifice of the *aorta* does to the whole internal surface of the left ventricle of the heart; *i. e.* supposing the area of the orifice of the *aorta* = 0.5 of a square inch and the internal surface of the left ventricle = 15 square inches\*, as 1 to 30; and therefore the force with which the blood is pushed into the *aorta*, must fall short of  $\frac{1}{3}$  of 60 pounds weight. Hence a resistance in the *aorta*, equal to two pounds, will require a force of above 60 pounds exerted by the whole internal surface of the left ventricle of the heart to overcome it: from which

\* *Hales* loc. cit.

which it follows, either that the resistance to the motion of the blood in the *aorta* and all its branches and ramifications must be less than two pounds, which I believe no body will affirm ; or else that the protrusive force of the left ventricle of the heart alone, is unable to drive the blood through all these vessels, and consequently insufficient, without the assistance of some other power, to carry on the circulation.

IF any one should, on this occasion, have recourse, with the learned *Borelli*, to the *vis percussiois*, we need only observe that the force of the heart, is evidently not a percussive, but a pressing one; so that, although the least percussive force may be greater than any finite quiescent resistance, yet this will not hold true of a pressing force, which, in order to have any sensible effect, must be greater than the resistance it has to overcome: to say otherwise, is to affirm that, with the pressing force of one's hand, the greatest mountain might be moved out of its place.



NOR is Dr *Keill*'s account of this matter more satisfactory, viz. that, the blood being once put in motion, a very small force in the heart may be sufficient to keep it always in this state: for this force must be equal to the loss of motion, sustained by the blood, in every circulation, and consequently to the resistance which this fluid meets with in its passage thro' all the vessels of the human body; a resistance by far too great to be balanced by the few ounces to which the *Doctor* has reduced the force of the left ventricle of the heart \*.

BUT that the foundation upon which Dr *Keill* proceeds is false, and that the heart can really communicate a new motion to the blood when the old one is in a great measure lost, and after all the fluids have been for some time almost entirely at a stand, is evident from the recovery of people who have lain for sometime in a *syncope*, and from the revival of the sleeping animals, which are, in appearance, dead all the winter-season. But further, since the blood,

when

\* Tentam. med. phys. 3. de vi cordis.



when it returns to the right ventricle of the heart, has scarce  $\frac{1}{10}$  of the force with which it was thrown into the *aorta*\*, 'tis plain that it acquires, every circulation,  $\frac{9}{10}$  of its force in passing through the heart and lungs.

THUS much being said to shew that the force of the heart is, of itself, not sufficient to carry on the circulation, we shall next briefly consider the alternate contraction of the *aorta* and its branches, which has been justly reckoned among the chief causes of the motion of the blood.

THE blood thrown out at every *systole* by the left ventricle of the heart, is not instantly transmitted through the capillary arteries into their corresponding veins, but the greatest part of it is accumulated in the now-dilated arteries, and is, during their succeeding contraction, conveyed on thro' the smaller vessels. This contraction however of the arteries may, perhaps, be considered, rather as a continuation of the heart's force, than as any new power impressed

\* *Hale's Statical Essays*, vol. 2.

impressed on or communicated to the blood; since it does not appear that the arteries contract with a greater force, than that by which they were dilated. But, whatever may be the force with which the *aorta* and its branches restore themselves, we know certainly that it is less than the systolic power of the left ventricle of the heart; because the blood is observed always to be projected to a greater distance from a cut artery during its *diastole*, than in the time of its *systole*. Whence it follows, that, if the force of the heart is insufficient to account for the motion of the fluids through the inferior orders of vessels, the alternate contraction of the muscular coat of the *aorta* and its branches must be so likewise. It is, however, to be observed, that the sanguiferous arteries, whose numerous branches are dispersed every where through the body, must not only, by their alternate contraction, contribute to push forward their contained fluids, but also, by their dilatation, so compress the inferior orders of vessels, as somewhat to promote the motion of the fluids

fluids in them\*. I shall only add on this head, that, as the alternate contraction of the arteries depends intirely upon their preceeding dilatation by the heart, so, in the serous and inferior orders of arterial vessels to which the projectile force of the heart seems not to reach, there is no such alternate dilatation and contraction to be observed†.

WITH respect to gravity, which some have reckoned among the causes promoting the circulation, it is sufficient to observe, that in a horizontal position of the body, it can have no effect; and, in an erect one, it must retard the return of the blood by the *vena cava inferior*, as much as it promotes its motion downwards in the *aorta* and its branches.

THERE is scarcely any thing that will sooner or more naturally strike the mind of one who inquires into the causes of the motion

\* Vid. *Edinburgh Medical Essays*, vol. 5. p. 2. edit. 3. p. 39. where this point is well illustrated by the ingenious Dr *Gilchrist*.

† *Leuwenhoeek* epist. 65. p. 167.

tion of the fluids in the very minute vessels of animals, as well as vegetables, than that surprising power of attracting liquors which capillary tubes are endowed with. But although the attractive power of capillary tubes may assist us in accounting for the imbibition of fluids by the vessels commonly called absorbents, as we shall afterwards have occasion to shew; yet it must appear evident to every one acquainted with the *phenomena* of these tubes, that this attraction can be of no use in promoting the circulation of the blood in the capillary arteries and veins: since these vessels are always full; or, if they were not, the fluids would be determined by it, equally backward towards the larger arteries as onwards to the veins.

## S E C T



## S E C T II.

*That the vibratory motion of the small vessels of animals is the principal cause promoting the circulation of their fluids.*

HAVING shewn the insufficiency of the powers already mentioned to account for the circulation of the fluids in the very small vessels of animals, we shall now proceed to explain what we imagine to be the principal cause of this circulation.

ALTHOUGH, as has been observed above, the regular alternate pulsation of the arteries does not extend beyond the capillaries of the first order, except, perhaps, in places very near the heart; yet we are not to consider the serous, lymphatic, and other still smaller vessels, as unactive canals no ways contributing to promote the circulation of their different fluids: on the contrary, it seems highly probable, that these vessels are continually agitated with very small alternate contractions, to which the circulation



tion in them is, in a great measure, owing.

MANY physiological writers have supposed an oscillatory motion in the small vessels of animals\*, but few have said any thing satisfactory concerning the cause of this motion. *Baglivi* supposed the membranous parts of the body to derive their oscillations from the *dura mater*; and the vascular system and fleshy fibres, theirs from the heart: but, as it is now past doubt that the *dura mater* has no other motion than what arises from the pulsation of its own vessels or those of the brain; and as the alternate contraction of the arteries depending upon their preceeding dilatation by the blood thrown out by the heart, has no place in the serous, lymphatic, and inferior orders of vessels; the vibratory motions of these  
canals

\* Among others, the learned *Dr De Gorter*, in his treatise *de motu vitali*, has not only admitted a vital oscillatory motion in the small vessels, but endeavours to shew, that, without this, the force of the heart would be unable to carry on the circulation, § lvi. &c.

canals must be deduced from some other cause.

MANY experiments and observations shew that the muscular fibres of animals are so framed, as to be readily excited into contraction by a *stimulus*. The small vessels, therefore, which are endowed with a muscular coat, as well as the larger ones, must necessarily be agitated with alternate contractions, as often as they are acted upon by any thing capable of gently irritating them; but such are the blood and finer fluids derived from it, which, while they slowly glide through the small vessels, stimulate their internal surface, so as to excite them into gentle but continually repeated contractions.

SOME of the greatest philosophers and physicians, of antient as well as later times, have imagined the blood to be a very active fluid, endowed with uncommon qualities, and, as it were, the fountain and source of life in animals\*: nor do they seem to have

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been

\* *Aristot. histor. animal. lib. 3. cap. 19; & Harvey de generatione animal. exercitat. li. lii. et lxxi.*

been led into this opinion so much from any favourite theory, as from experiments and observations made on living and dying animals. But, without entering into, much less defending, the peculiar notions of those authors concerning the blood, we shall only say, that this fluid is extremely well fitted to act as a gentle *stimulus* upon the sensible fibres of animals, whether we consider its composition, heat or intestine motion: for, while the saline and other acrid particles in the blood render it fit to irritate the tender vessels, its heat and intestine motion keep all its parts in a perpetually vibrating state, which must increase their stimulating power †. Agreeably to this, we find, that, in many insects and some larger animals, the circulation becomes more languid, as the weather grows colder, and, in the winter season, is altogether at a stand, till, by the heat of the returning spring, the particles of the fluids begin to be briskly agitated, and consequently the solids stimulated into contraction.

† See an Essay on the vital and other involuntary motions of animals, sect. 3.

contraction. Doctor *Harvey* has long since remarked, that the hearts of several shell-fishes are only seen to beat in warm weather † ; and the curious observations of *Reaumur* have shewn us, that the lives of insects may be lengthened or shortened, and made more or less active, by exposing them to different degrees of heat and cold ‡.

Thus much being said to shew, that the blood is well fitted to act as a *stimulus*, we shall offer some further considerations to prove, that the small vessels are, by its influence, really excited into alternate contractions. And,

I. WE are led to conclude this from what we observe in the larger canals and vessels of animals. Thus the several portions of the intestinal tube are solicited into alternate contractions by the aliment, air, and bile, stretching their coats and stimulating their internal surface : and, as we imagine an alternate motion in the small vessels necessary to promote the circulation of the

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fluids

† De motu sang. cap. xvii.

‡ Histoire des insectes, tome 2. memoire 1.



fluids in them, so we know certainly, that the peristaltic motion of the guts is the principal cause which conveys the digested aliment down towards the *anus*.

Not only the auricles and ventricles of the heart, but also the trunks of the *venae cavae* adjoining to the right *sinus venosus*, are continually agitated with alternate contractions\*. The trunks of the *venae cavae* preserve this motion, in animals newly dead, a considerable time after the pulsation of the heart has ceased; but no sooner is the blood contained in these vessels evacuated, and all new supplies intercepted by ligatures, than their sides collapse, and remain without the smallest motion†: whence we are led to conclude, that the alternate contractions of these veins are, like those of the heart, owing to the blood acting upon them as a *stimulus*.

It is generally allowed by physiologists, that the *systole* of the larger sanguiferous arteries, in which a remarkable pulsation obtains,

\* Essay on vital motions, &c. p. 97 and 354.

† Bartholin. epist. cent. iv. p. 109, &c.



tains, is owing, not only to their elasticity, whereby they endeavour simply to recover themselves, but partly also to a proper muscular contraction of their tendineo-carnous coat\*: and, as this is excited by the blood pushed into them by the heart, which, at the same time that it distracts their fibres, gently irritates their internal surface; it seems highly reasonable to allow, that the smaller vessels, endowed at least with equal sensibility, must be excited into feeble but continually repeated contractions, by the gentle *stimulus* of their circulating fluids.

FURTHER, as there are some of the more imperfect animals which have no heart, the circulation in them must be owing to the contractile power of the vessels themselves excited into action by the *stimulus* of the fluids. And that the vessels of those animals, which, in a natural state, have a heart, are endowed with a similar power, seems

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proved

\* The diminution of the strength of the pulse in an arm that is quite palsied, is a strong proof that the larger arteries act partly by a muscular power.

proved by examples of monsters wanting a heart or any thing analogous to it\*, in whom the fluids must have circulated chiefly by the power of the vessels.

2. A variety of facts might be mentioned, which clearly demonstrate an alternate contractile power in the small vessels of animals, and that this is exerted more or less according to the degree of irritation affecting them.

<sup>a</sup> Thus, the steams of warm spirit of wine received into the eyes, not only cause a greater flow of tears from their vessels, but, in a few seconds, produce an artificial inflammation in them, that is, they make the globules of red blood enter the serous or lymphatic vessels of the *conjunctiva*. Now, as this additional moment of the blood, whereby it is enabled to dilate these vessels, cannot proceed from the heart or larger arteries, since their force is not, nor can be altered in

\* Vid. Van Swieten comment. in Boerhaave aph. vol. 1. p. 256; and histoire del'acad. des sciences, 1703; & memoires, 1740.

in the present case ; it must be owing to the extraordinary alternate motion excited in the vessels of the eye by the steams of the spirit of wine.

I presume it will not be alledged, that the vapour of spirit of wine raises an inflammation in the eye, by constringing its vessels so as to occasion an obstruction in them, and that this obstruction afterwards produces the inflammation, by lessening the number of vessels through which the blood passes, and consequently increasing its force upon the obstructed ones : for, not to insist on what might be easily proved, that no obstruction can ever produce an inflammation except in so far as it gives rise to an unusual irritation ; the spirit of wine should, by contracting the serous and lymphatic vessels of the *conjunctiva*, enable them to sustain this additional force.

BUT further, why does tepid milk and water, or a poultice of bread and milk, lessen an inflammation of the eye, while acrid astringent and spirituous things increase it ? According to the doctrine of inflammation

tion from mere obstruction, together with an increased force of the heart and larger arteries, one would think that the former should, by relaxing the small vessels, expose them to be still more and more dilated by the increased force of the blood, and so increase the inflammation; while the latter should, by contracting those vessels, enable them not only to resist the blood impelled by the heart, but also to expell the obstructing red globules. But the truth of the matter is, that the tepid milk and water and poultice, by relaxing the vessels, lessen or remove the irritation and sense of pain, which, by raising uncommon contractions in the small vessels, was the cause of the inflammation; while acrid astringent and spirituous applications, though they tend to contract the vessels, yet, by increasing their vibratory motions, greatly augment the force of the blood in them, and therefore must necessarily increase the inflammation.

§ THE heat, redness and inflammation, brought on the skin by blisters and sinapisms, are not owing to any increase of the heart's force,



force, or of the moment of the blood in the larger vessels, though this is often an effect of their application ; but merely to the action of those irritating substances on the cutaneous vessels, whereby the motion of the fluids in them is greatly augmented.

γ THE sudden redness and glowing warmth of the face, which, in women especially, accompanies a consciousness of shame, and is commonly distinguished by the name of *blushing*, can only be satisfactorily accounted for, from an increased motion of the small vessels of the face.\*

δ THE extraordinary flow of spittle which happens to hungry persons from the sight or even the remembrance of grateful food, and the profuse secretion of urine which hysterical people are frequently subject to, cannot be explained without having recourse to an increased motion suddenly excited in the small vessels of the salivary glands and kidneys; and clearly shew that the quantity of spittle and urine separated by these organs,

\* See an Essay on the vital and other involuntary motions of animals, p. 101. and 102.



does not depend so much upon the force with which the blood is determined into their vessels by the heart, as upon the greater or lesser vibratory motions of the secerning vessels themselves. And in the same manner, is it not reasonable to believe, that the motion of the fluids in the smallest vessels every where through the body, is as much, perhaps more, owing to their gentle alternate contractions, than to the force of the heart and larger arteries?

THE secretion of tears, which is very little affected by the different forces with which the blood is impelled by the heart, is immediately increased in a very great degree by acrid applications to the eyes, or by certain passions of the mind.

In the first case, the greater secretion is owing to the acrid matter, which, by its irritation, raises an uncommon alternate motion in the lachrymal vessels. Nor can it be, with reason objected here, that acrid things applied to the eyes or received into the mouth, occasion a greater flow of tears or spittle, not by raising any stronger motion.

motion in the small vessels of the lachrymal and salivary glands, but merely by constringing their excretory ducts, and so squeezing out the liquors contained in them; since the quantity of tears and spittle discharged in such cases shews, that not only the excretion but the secretion in these glands is greatly increased. And if an irritation of the *pelvis* of the kidney, or *ureter*, from a stone lodged there, often occasions an uneasy sensation in the extremity of the *urethra*\*; is it not reasonable to think, that, upon the application of stimulating things to the orifices of the lachrymal and salivary ducts, these will not be affected alone, but the irritation will, in some degree, be communicated to the small secretory vessels of their respective glands, so as to excite in them stronger and more frequently repeated contractions, and consequently increase their secretions?

THE flow of tears which accompanies certain affections of the mind, is, like the greater

\* Van Swieten Comment. in Boerh. Aphor. vol. i. p. 301. et Morton de Pthisi, lib. ii. c. 3.

greater secretion of spittle from the sight of grateful food, and the heat and redness of the face from a consciousness of shame, owing to an unusual vibratory motion excited in the lachrymal vessels in consequence of these affections; and not to any compression which the lachrymal gland may suffer from some of the neighbouring muscles, which are then brought into contraction; for no degree of alternate compression applied to this gland remarkably increases the secretion of tears, unless its vessels, or those of the eye, are thereby irritated.

3. We have already seen, that an increased alternate motion in the small vessels occasions a quicker flow of liquors through them: and the following case will shew, that, when this motion is much diminished or wholly suspended, these vessels collapse, and the circulation in them either becomes very languid, or ceases altogether.

A boy betwixt four and five years of age was, on *Saturday* afternoon, suddenly seized with an apoplexy or abolition of  
sense

sense and voluntary motion. On *Sunday* morning, at nine o'clock, when I first saw him, his pulse was full and quick, and his eyes had something of a glazed look ; but in the evening this was more remarkable. *Monday* a little before noon, he was still alive, but his breathing was very laborious and his pulse small and quick ; at this time, his eyes were more shrivelled, than they use to be in those who have been several hours dead.

THIS glazed appearance of the eyes could not be owing to the diminution of the heart's force, since the pulse was full and strong for twenty-four hours after the disease came on : nor can the failure of the pulse, afterwards, account for the eyes appearing more shrivelled than is usual in persons newly dead. But if the circulation of the fluids in the small vessels be chiefly owing to a vibratory motion in them, and and if this must cease when the influence of the nerves is intercepted ; in this boy, whose brain, especially its anterior part, was so remarkably obstructed, the motion of

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the fluids in the very small vessels of the *cornea* and the secretion of the aqueous humour must have been greatly diminished, and hence the dimness and shrivelling of the eyes \*.

THE withering of a member that is palsied, or deprived of the nervous power, is to be accounted for in the same manner; and is a proof that the circulation of the fluids through the inferior orders of vessels, is not more owing to the force of the heart, than to the action of these vessels themselves. This withering of a palsied member has made some imagine, that nutrition is performed by the nerves: but the *phenomenon*, we see, is easily accounted for without this supposition; and there are good reasons to think that the nerves are *solely* subservient to motion and sensation.

4. ALTHO' the alternate contractions of the smaller vessels, which we have been contending

\* Dr Nuck observed the secretion by the glands to be much diminished, or intirely stopt, after their nerves were obstructed or compressed. *Vid. Adenograph. curios.* p. 16.



contending for, are not remarkable enough to be discerned in most animals; yet they may be clearly seen in the legs of a bug: in the small vessels of which, an extraordinary vibration is discovered by the microscope †.

5. *Lastly*, THE vibratory, tho' invisible motion of the small vessels, is greatly confirmed, by that irregular motion, backwards and forwards, of the globules of blood in the capillary arteries, formerly observed in dying animals by *Lewenboeck*, and lately described more accurately by the learned *M. de Haller* \*. As this motion continues after the heart is cut out of the body, it cannot be owing to any impulse of the blood communicated from the larger arteries: and we have no reason to ascribe it to any *peculiar* attraction between the globules of blood ‡, since this fluid, neither when received into small glass tubes,

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nor

† Baker on the microscope, p. 130.

\* Aët. Gottingenf. Vol. IV. p. 351.

‡ Ibid. p. 354.

nor in any other experiment shews such a power.

This oscillation, however, of the blood globules, may be accounted for, from an irregular vibratory motion in the sides of the small vessels. We see that in animals newly dead, the *vena cava* is excited into alternate contractions by the *stimulus* of the blood contained, and that the fibres of the muscles, upon being exposed to the air, or the action of other *stimuli*, are frequently agitated with a weak irregular and tremulous motion; it is therefore reasonable to conclude, that the small arteries, which are of a similar nature with the *vena cava*, and whose power of motion in living animals shews them to be in some degree muscular, may, after the circulation ceases, by the *stimulus* of the cold air, or of the globules of blood contained in them, continue to be excited into small but irregular contractions, which tho' not observable even by the microscope, yet are discovered by their effects: for it is easy to see, that by the smallest contraction of the sides of a capillary artery, the

the contained blood will be put in motion, which will be renewed as often as such contractions happen.

This is still farther confirmed from its having been observed, that a few globules of blood extravasated between the *laminae* of the mesentery ascended and descended irregularly, and were agitated with the same kind of oscillatory motion as in the small arteries \* : for as often as by the least agitation in the air or other cause, the *laminae* of the mesentery approached nearer to one another, the globules would fly from that place; and would return to it again as soon as they receded from each other, in much the same manner as water suspended between two panes of glass is observed to ascend or descend, just as these panes are brought nearer to, or removed farther from each other.

THE objection against the reality of a vibratory motion in the small vessels of animals, because the microscope shews no such thing in most animals, is of no great

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weight;

\* Vid. Aët. Gotting. Vol. IV. p. 354.

weight ; since it cannot be doubted, that the particles of all bodies, especially fluids, are affected by heat with a perpetual oscillatory motion ; and yet, unless the heat be great, the eye, even assisted by the best microscopes, cannot discern any such thing.

FURTHER, since the microscope only shews the circulation of the fluids in the red capillary arteries, but not in the serous, lymphatic, and many inferior orders of vessels, can it be expected that any alternate vibratory motion should be discovered in these vessels ? Or, is it reasonable to deny an alternate motion to all vessels or particles of matter, which are too small to fall under the notice of our senses ?

ALTHO' the branches of the vine were transparent, so that the motion of the sap in its vessels could be seen by the help of a good microscope ; yet it is very probable we should not be able to discover any vibratory motion in them : and yet the force of the sap in the bleeding season shews, that, besides attraction, there must be a real propelling



propelling power exercised by the vessels of the vine\*.

If the diameter of the *aorta* in its *diastole* does not exceed its diameter when contracted above  $\frac{1}{3}$  of a line, i. e.  $\frac{1}{3}$  of its diameter†; and if the change of diameter, which happens in the red capillary arteries and inferior orders of vessels from their vibratory contractions, be three times less in proportion to the magnitude of these vessels than the difference of diameter in the *aorta*, arising from its alternate *diastole* and *systole*; then the difference between the greatest and least diameter of a capillary artery capable of receiving only one globule of red blood, when most dilate or contracted, will be equal to  $\frac{1}{1500}$  part of its diameter: i. e. supposing its diameter  $\frac{1}{1000}$  of an inch,  $\frac{1}{1000000}$  of an inch; and the space described by each side of such an artery, when it performs one of its small vibratory contractions, will be only equal to  $\frac{1}{2000000}$  part

\* Vid. *Haller's Statical Essays*, vol. i.

† Vid. *Weitbrecht in Comment. Academ. Petropolitani*. vol. vii. p. 314.

part of an inch, which is greatly too small to be discerned by the best microscope.

HAVING thus endeavoured, by a variety of arguments, to shew, that the small vessels of animals are, thro' the gentle *stimulus* of the fluids, continually agitated with alternate contractions; we shall now, briefly, point out their use in carrying on the circulation. And it must appear evident to every one, that the inferior orders of vessels will not only not retard the motion of the fluids, but greatly promote it; since every small portion of them will, like a little heart, by its alternate contractions, push on its contained fluid. Nor ought these contractions, however weak and imperceptible, to be thought unable to produce this effect; since the motion of the fluids in the very small vessels is far from being rapid, and just such as might be expected to arise from this cause. Dr *Hales* has observed, that, in a capillary red artery in one of the muscles of the *abdomen* of a frog, the blood moved only an inch in a minute and a half\*:

half\* : and it is probable, that, in the finest secretory vessels of the brain, the fluids may not move above a Parisian line or  $\frac{1}{12}$  of an inch in a minute, *i. e.* not twice

\* Statical Essays, vol. ii. p. 68. *Lewenhoeck* and the illustrious *Senac* have also observed the motion of the fluids to be very slow in the small vessels. On the other hand, *M. de Haller*, in his late treatise on the motion of the blood, tells us, that he has frequently seen this fluid moving faster in a small arterial branch in the mesentery of a frog, than in the trunk whence it took its rise (a). But we cannot conclude from this, that the velocity of the blood is greater, in a natural state, in the small branches than in their trunks; for this is repugnant to the most certain observations, which shew, that the capacity of the branches always exceeds that of the trunks from which they proceed. All therefore that can be fairly deduced from *M. de Haller's* observations is, that the blood may move as fast or even faster in some of the small arterial branches than in the trunks from which they take their rise, as often as those branches are affected with an unusual irritation, or the other branches proceeding from the same trunk are obstructed or contracted by cold or other causes.

(a) *Aët. Gotting.* vol. iv. p. 294. 295, and 299.

twice as fast as the *minute* hand of a small-sized watch.

If it be objected, that, as the capillary arteries and veins are destitute of valves, their alternate contractions must push the fluids equally back towards the heart, as onwards to the larger veins: it may be sufficient to answer, that the resistance arising from the semilunar valves of the *aorta*, and from the force of the heart and larger arteries *a tergo*, being greater than that which opposes the transmission of the fluids into the larger veins; the fluids acted upon by the small vibrating vessels, must necessarily be determined towards the latter. But further, why may not the alternate contractions of the small vessels, like the peristaltic motion of the intestines, proceed in such manner, as to impell their fluids more remarkably onwards to the veins than backwards to the larger arteries?

UPON the whole, as we conceive the motion of the blood in the larger vessels, and even capillaries of the first order, to be  
owing



owing to the alternate *systole* of the heart and arteries ; so in the serous, lymphatic, and still smaller vessels, where this force either reaches not at all, or is greatly diminished, the circulation seems to be carried on, chiefly by the vibratory motions of these vessels themselves : and, the finer fluids being, in this manner, transmitted into the larger veins, the pulsation of the neighbouring arteries, action of the voluntary muscles, and alternate compression made upon all the contents of the *abdomen* and *thorax* by the motion of respiration, will promote their return to the heart along with the red blood in the *venae cavae*.

WHAT we have said of the circulation of the fluids in general, we would have understood also of their motion in the secretory pipes of the several glands. In those glands whose vessels are most patulous, the secretion may be partly, and indeed principally, carried on by the force of the heart and larger arteries ; a proof of which seems to be the bloody urine passed by such as have weak kidneys, after violent exercise :

exercise: but in other glands, whose structure is finer, and particularly in the brain and *testes*, the motion of the fluids in the secretory and excretory vessels seems to be much less owing to the force of the arterial blood *a tergo*, than to the gentle vibratory contractions of the vessels themselves.

With regard to the nerves, which are generally considered as the excretory ducts of the brain; it is probable, that the derivation of their fluid to the various parts of the body is not only owing to a gentle oscillatory motion in them and their surrounding membranes, but also, in some degree, to their attraction as capillary tubes; for no sooner can there be a waste of this fluid at the extremity of any nerve, whether this happens from exhalation, alternate compression of the neighbouring parts, or any other cause, than, by its attractive power, it will be filled again. In the other glands, however, whose excretory ducts, by their union, soon form pretty large canals, no such attraction will have place.

1. FROM what has been said, it may appear, that we are not to consider the force of the heart and contraction of the larger arteries, as the sole causes of the circulation of the fluids in animals. The whole vascular system is endowed with a moving power, which is constantly excited into action by the *stimulus* of the circulating fluids; so that while the small vessels, by means of friction, destroy in part the moment of the juices, they, at the same time, communicate, by their gentle vibratory contractions, a new impulse to them. Every part therefore of the vascular system, as well as the heart and larger arteries, nay every section even of the smallest vessel, is to be conceived as promoting the circulation of the fluids; that great work, upon which the life of the whole depends, and, in carrying on which, every part almost of the body is active.

2. If the motion of the fluids in the inferior orders of vessels be not so much owing to the force of the heart and larger arteries as to the gentle alternate contractions of those vessels  
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themselves,

themselves, we may easily see, why frictions, warm, penetrating, and stimulating fomentations, and cataplasms, &c. are often more successful, than internal medicines, in removing obstructions in the serous, lymphatic, and other small vessels; since they not only contribute to attenuate the obstructing matter, but greatly increase the oscillatory motion of these vessels. For the same reason it is, that the warm mineral waters, pumped with considerable force, upon a part affected with the rheumatism or *sciatica*, have effected a cure after other remedies had been used in vain.

WARM spirit of wine, either alone or mixed with other things, proves often a good decoobstruent when applied externally: yet I have known some people who were afraid to use it with this intention, because it is known to coagulate the *serum* of the blood: but their fears were without foundation; for the quantity of spirit of wine, which enters by the pores of the skin, is so small as to be in no danger of producing any *coagulum*; besides, as it is taken in by the absorbent



forbent veins, it must go to the heart and be mixed with the mass of blood, before it can come at the obstructed vessels, unless when the obstruction happens to be in any of those glands, in which the valvulous lymphatics terminate; for since my very ingenious Collegue Dr MONRO junr. has proved these lymphatics to be no more than absorbent veins,\* they must carry the finer parts of such substances as are applied to their mouths, directly to those glands which they enter, and before they can be mixed with the mass of blood. But, altho' little is to be expected from the resolving, or to be dreaded from the coagulating power of the spirit of wine, yet it proves, in many cases, a good deobstruent, by raising an uncommon oscillatory motion and heat in the vessels of the part to which it is applied.

3. If the circulation in the small vessels be, in a great measure, owing to their vibratory motion excited by the stimulus of the circulating fluids, it will follow, that, when these vessels, in any part of the body, are affect-

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\* Vid. dissertat. de venis lymphaticis valvulosis.

ed with an extraordinary irritation, they must necessarily be agitated with much stronger and more frequently repeated contractions than usual: whence the force of the blood in them will be greatly increased; and the small arteries will not only be, more than usually, distended with blood, and consequently the part inflated, but the red globules will be pushed in to the serous vessels,\* and in many cases will be forced into the spaces of the *tela cellulosa*, through the dilated orifices of the small arteries, which terminate in them †; and this must happen, whether the force of the blood be, or be not increased

\* Although an inflammation *ab errore loci* may not happen so often as has been alledged, yet the inflammation of the *cornea* and *conjunctiva* covering it, whose vessels in a natural state do not admit red globules, is a sufficient proof that inflammations have not only their seat in the red capillaries, but also in the serous arteries. Nay the effusion of blood into the spaces of the *tela cellulosa*, is itself a proof of an *error loci* in inflammations, since this effusion is much seldomer owing to a rupture of the small red arteries, than to a dilatation of the orifices of those vessels which in a natural state, only transmit a thin, colourless fluid.

† Haller. Element. Physiolog. tom. 1. lib. 2. sect. 30.

increased in the other vessels of the body. An inflammation, therefore, is not owing to an increased force of the heart and larger arteries consequent upon an obstruction, as some authors of great name have imagined; but to an increased alternate contraction in the small vessels, whether this arises from some obstructing matter distracting their fibres, or acrid matter irritating them. An obstruction without an irritation in the obstructed part, never occasions an inflammation; but the irritation of any sensible part with a sharp instrument, or acrid matter, never fails to produce this effect, although there be no preceeding obstruction, nor increase of the heart's force. When a large artery is tied in the operation of the aneurism, we don't find, that the increased momentum of the blood in the neighbouring arteries, produces an inflammation in the arm; but, when a tendon is wounded in blood-letting, or a little acrid matter is collected below the nail, a remarkable pain, swelling and inflammation follow.

HOWEVER, altho' an increased force of the

blood in the large vessels is not the cause of an inflammation, yet it is frequently the consequence of it: for, as often as the inflammation is large, or the part inflamed very sensible, the whole nervous system will be so affected by the pain, as to render the heart and larger arteries more irritable, at the same time that the blood, now vitiated by the obstruction and inflammation, must act upon them as a stronger *stimulus* than usual. Hence we may see, why, in inflammations, the pulse is often little changed till the disease has continued for some considerable time. In inflammations of the stomach, intestines, and *uterus*, the pulse, though much quickened, often continues small; because, on account of the particular sympathy between their nerves and those of the heart, this muscle is rendered so irritable, as to contract before its ventricles are filled with the returning venous blood.

FROM what has been said it may appear, that, in the cure of inflammations, besides diminishing the force of the circulation in general by blood-letting, a particular regard



gard is to be had to the vessels of the part affected, whose extraordinary alternate contractions, should be lessened by proper emollient and anodyne applications, and, in many cases, by blistering the neighbouring parts. My ingenious friend Dr *Pringle* has often observed the good effects of blisters, even when early applied, in pleurisy and other internal inflammations\*. And I have frequently seen a blister, in fifteen hours, lessen remarkably the quickness of the pulse, not only in obstructions of the lungs attended with a fever and considerable expectoration of phlegm †, but also in pleuritic cases, and in an *angina*, after bleeding once and again had done little this way; nay, in obstructions of the lungs and pleurisy I look on it as one of the worst signs, when after proper bleeding a large blister does not lessen the quickness of the pulse; for I have rarely seen any such cases that did not prove fatal. I know many physicians have

\* See his observations on the diseases of the army, 1<sup>st</sup> edit. p. 173, 178, and 179.

† See Philosophical Transact. vol. L. part. ii. p. 569.

have entertained prejudices against blistering in inflammations, because, by their irritation, they increase the force of the circulation in general : but, not to mention the good effects they may have by attenuating the obstructing matter, and making a considerable derivation of serous humours from vessels which are nearly connected with those of the part affected; if the account we have given of inflammations be true, it must follow, that altho' the *material* cause of an inflammation, *i. e.* the acrid or obstructing matter, be not immediately removed by blistering ; yet, if, according to *Hippocrates's* observation \*, the painful sensation in the inflamed vessels be lessened by its means, the extraordinary alternate motions of these vessels, and consequently the cause continuing and increasing the inflammation, must be also lessened. Hence it appears, that a blister, though it tends to increase the force of the circulation in general, may yet lessen the *impetus* of the blood

\* Δυσὶ πόνον ἀμὰ γινόμενον, μὴ κατὰ τ' αὐτοὺς τόπους, ὁ σφοδρὸς πόνος ἀμικρὸς τ' ἴσχυρον. Duobus doloribus simul, obortis, non in eodem loco, vehementior obscurat alterum. Aphor. lib. 2. No. 46.

blood upon the vessels of an inflamed part more remarkably, than even blood-letting itself. In patients, in whom there is no fever or increase of the circulation from any inflammation, blisters are observed, by their *stimulus*, to raise the pulse and augment the heat of the body; but in internal inflammations, after such bleeding as the circumstances of the patient may require, blisters often abate the fever and heat of the body, as well as the quickness of the pulse, by lessening or removing the inflammatory obstruction.

WHAT has been said of blistering, may be applied also to cupping and scarifying in pleurifies, *angina's*, &c.

SINAPISMS, laid to the soles of the feet, remove or lessen ravings, not by determining the blood more copiously to the inferior extremities, for their effect in this respect is altogether trifling; but by raising a very considerable pain, which so affects the mind, as to render it less sensible of the unusual *stimulus*, or irritation in the brain, or its membranes, *i. e.* of the cause producing and continuing the *delirium*. Nor is it material to what part

part of the body those cataplasms are applied; for a strong *delirium*, in a fever, has been removed by the application of a sinapism, by mistake, instead of a poultice of *theriac*, to the region of the stomach.

We may also, from what has been said, see how ravings, phrensies, and madness have been cured by the power of music\*, or by a sudden fright†; for these, by greatly affecting the mind, and fixing its attention, not only render it less sensible of the disordered state of the brain and its membranes, but, by the strong impression they make on the *sensorium commune*, may tend to dislodge or remove the cause of the disease.

### S E C T III.

*Of the motion of the fluids in those vessels of animals commonly called absorbent.*

BESIDES the small veins, which are continued vessels with the arteries, and terminate

\* Histoire de l'acad. des sciences, 1708 & 1717.

† Van Swieten comment. in Boerhaave, aph. § 11.



nate at last in the two *venæ cavæ*, there are others which take their rise from the internal surfaces of the several cavities in the body and from the skin: and, as the fluids which these vessels convey, cannot be impelled into them by the force of the heart or arteries, they have been thought to receive them by suction, and therefore have got the name of absorbent or imbibing veins. In the intestines we find two kinds of them, viz. the lacteal veins, and those commonly called absorbent; which last are also to be found upon the surface of the skin, *peritoneum*, *pericardium*, *pleura*, vesicles of the lungs, *dura* and *pia mater*, and, in short, of every membrane which lines any cavity of the body. In accounting for the motion of the fluids in these vessels, we shall begin with the lacteals; in order to which it may be necessary to premise,

I. THAT the lacteal veins have their origin in the villous coat of the guts, where their orifices are so small as to escape the eyes of anatomists: that leaving the posterior surface

face of the villous, they pass through the nervous and muscular coats, and, uniting into larger canals, are distributed in the form of a net-work in the external cellular membrane of the intestines; and that after this, they enter the mesentery, and are furnished with numerous valves, which hinder the return of any thing to the intestines.

2. As often as the muscular coat of the guts is contracted, the lacteal veins, which pass between the interstices of its fibres, and are distributed in the nervous and external cellular membranes, must necessarily be compressed; but are relaxed and freed from this pressure, when this coat ceases to contract.

3. MANY and repeated experiments have shewn, that small glass tubes are endowed with a power, by which they attract fluids, so as to raise them considerably above the liquors in which they are immersed—That this power increases exactly in the inverse ratio of their diameters—That these tubes, whether straight or crooked, in a perpendicular or oblique position, in *vacuo* or the open

open air, attract fluids to the same height, provided their diameters be equal.—That, when a capillary glass tube ends in a larger canal, the fluid is elevated so as to fill the capillary, but does not ascend any further.—That, if the diameter of a glass tube exceeds  $\frac{1}{16}$  of an inch, its power of attraction is scarcely perceivable: and lastly, that the same glass tubes attract different fluids to different heights, and this neither in proportion to their tenacity nor gravity. From all which it is natural to conclude, that the lacteal veins, which, in their beginning at least, are smaller than any glass tubes made by human art, must be endowed with a remarkable power of attracting the chyle, when applied to their orifices.

How far the attractive power in such canals, as the lacteals and other absorbent veins, is, *ceteris paribus*, greater or less than in glass tubes, we have no experiments to determine: but, as the urine, an animal liquor, is more strongly attracted by glass capillaries, than water or any o-

ther fluid \* ; it is not unreasonable to suppose that animal capillaries may be endowed with a still stronger power of attracting it. And, as the same fluid is differently attracted by capillary tubes of different natures, tho' of the same diameter † ; is it not probable, that the several absorbent veins in animals may be peculiarly fitted to attract their proper liquors most strongly ?

FURTHER, the remarkable attractive power with which the small vessels of vegetables are endowed, and by means of which they draw from the same moist earth very different juices, is a strong argument for allowing a similar attraction in the vessels of animals. It is by this power that the sap continues to rise in the vessels of trees, even in the cold season of winter, tho' slowly and in small quantity : nor can it be pretended, that the sun's heat promotes the ascent of the sap here, as it does in summer ;

\* Muschenbroeck de tub. capill. vitr. cap. 3.

† Muschenbroeck. Element. philos. natural. cap. xviii. § 531.



summer; since trees in cold cloudy weather, provided it be dry, and in places which the winter sun-beams cannot reach, take up continually, by their roots, as much moisture as is necessary to supply the waste by perspiration in their trunks and branches. But further, Dr *Hales* has observed, that cut branches will imbibe from the small end immersed in water to the great end, as well as from the great end immersed in water to the small end \*: whence it clearly follows, that the ascent of the sap in the vessels of plants, is not owing to any peculiar structure in them, but solely to capillary attraction.

'Tis true indeed that capillary attraction, tho' it must make the sap rise in plants, will not, without the assistance of some other cause, make a continued derivation of it from their roots to their branches and leaves; because as soon as capillary tubes are filled, or have raised fluids to a certain height, all motion from attraction ceases: but as the action of the air

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and

\* Statical Essays, vol. 1.

and sun-beams upon the trunks, branches and leaves of trees, occasions a strong perspiration of the sap by their pores; a proportional quantity will be attracted from the earth by their roots, to supply this waste and keep the capillary vessels always full. However, as often as the absence of the sun and the cool moist state of the air put a stop to the perspiration of vegetables, the sap ceases to ascend; nay, if the earth be warm and dry, the sap gets a retrograde motion: and hence it is, that, in a cool summer's evening when the dew begins to fall, vegetables attract the watery particles in the air by the pores of their leaves and branches, in like manner as they had done the moisture of the earth by their roots, in the day time \*.

THESE things being premised, it will be easy to account for the imbibition of the chyle by the lacteal veins.

WHEN any proportion of the intestines is relaxed, the lacteal vessels, whose open mouths are every where to be found on the surface

\* Vid. *Hales's Statical Essays*, vol. I.

surface of the villous coat, take in the chyle by their attractive power, so as to fill their branches which are dispersed in the nervous and external cellular membranes of the gut. The chyle being thus received into the capillary lacteals, is, by the succeeding contraction of the muscular coat of the intestine, which compresses them, pushed on towards the mesentery. As soon as this contraction ceases, the emptied lacteals, being free from compression, fill themselves with chyle as before, which the succeeding contraction of the gut presses forward to the larger lacteals in the mesentery. And thus we see the chyle is by turns attracted and propelled by the capillary power of the lacteals and peristaltic motion of the intestines.

FURTHER, it is probable, that the lacteal veins are, like the other small vessels of animals, agitated with a vibratory motion, excited in them by the gentle irritation of the chyle, which must assist the alternate contractions of the intestines in the propulsion of this fluid. Without allowing

such a vibratory motion in the umbilical veins of the chick, it will be no easy matter to account for its growth during the time of incubation. 'Tis true, the umbilical arteries and veins run close together in oviparous as well as viviparous animals, so that the alternate pulsations of the former must contribute to the propulsion of the fluids in the latter towards the heart. But, as there is no pulsation to be observed in the heart or umbilical arteries of the chick, till towards the end of the second day \*; and as, at any rate, the pulsation of the umbilical arteries does not extend beyond the red capillaries; the fluids in the extreme branches of the umbilical vein, must owe their motion to some other cause. And is it not reasonable to think that the colliquated white is conveyed thro' these vessels by their attractive power, as capillary tubes, assisted by the small alternate contractions excited in them by the gentle *stimulus* of this warm fluid? And in this opinion we are confirmed by the analogy of plants; in whose vessels the circulation of the

\* Malpigh. de ovo incubato.



the sap is greatly assisted by a vibratory motion, which seems to be excited in them chiefly by the sun's heat. And is not the remarkable force of the sap in the bleeding vine, owing to its vessels being susceptible of much stronger vibrations than those of most other plants?†.

THE chyle in the larger lacteal veins which run along the mesentery, and are provided with valves, is pushed on to Pecquet's receptacle by the force of the new chyle continually transmitted to them from the guts, by the pulsation of the sanguiferous arteries which run contiguous with them, and by the alternate motion of the diaphragm and abdominal muscles in respiration.

IF the chyle is received into the nascent lacteal veins of the guts by their attraction as capillary tubes, it will be easy to see why quick-silver, which is repelled by such tubes, should,

† Dr Hales has observed, that, in a stem of a vine  $\frac{3}{4}$  of an inch diameter, the force of the sap in the bleeding season was five times greater than the force of the blood in the crural artery of a horse. Statical Essays, vol. I. exp. 36.

should, when swallowed by itself, generally pass through the intestines without, almost, any of it getting into the blood. On the other hand, if the propulsion of the chyle is owing to the alternate contractions of the guts, it may easily appear, why it ceases to be transmitted through them soon after death; and why, in a well-fed animal newly killed, the lacteals in the mesentery, after being emptied, may be filled again, by gently pressing the intestines and imitating their peristaltic motion.

WITH respect to the absorbent veins of the intestines; the finer parts of the digested aliment received into them by their attraction, are propelled towards the larger meseraic veins and *vena portarum*, by the alternate contractions of the muscular coat of the intestines and pressure of the abdominal muscles and diaphragm in respiration. But, as these absorbents are not provided with valves, like the lacteals, it may be asked, Why the last-mentioned power does not press the absorbed fluids equally backward to

to the guts, as forward to the *vena portarum*. This we imagine is prevented,

1st, By the gentle alternate contractions of the absorbent veins, which, as they are owing to the *stimulus* of the imbibed liquor, must begin at their orifices, and proceed towards their larger trunks. Such a motion as this, tho' gentle, will determine the course of the fluids on to the larger veins, but oppose their return to the intestines. And we find in fact, that, by means of a similar motion in the intestines, the useless part of the aliment is conveyed to the great guts, even in a horizontal position of the body, where the alternate pressure of the diaphragm and abdominal muscles ought to push the contents of the bowels as much backwards to the stomach as forwards to the *colon*. But,

2. WHEN any portion of the intestines is contracted, the nascent absorbent veins, which rise from the villous coat and pass thro' between the other membranes of this part, must have their sides pressed together, so as to allow nothing to pass through them; wherefore

wherefore the pressing force of the muscles of respiration must, if acting at this time upon the larger trunks of the absorbent veins, propell their fluids towards the *vena portarum*. When this portion of the intestine is relaxed, the emptied absorbents will, by their attraction, greedily fill themselves with new fluids from its cavity : so that, whether the guts are contracted or relaxed, there will be always some obstacle to the retrograde motion of a fluid in the absorbent veins.

WHEN the liquors taken up by the capillary absorbents are conveyed into the larger meseraic veins, they will be carried along with *their* blood to the *vena portarum*.

As there are, upon the internal surfaces of all the cavities of the body, exhaling arteries which perpetually throw out a fine fluid to moisten and lubricate the parts; so there are bibulous veins which take it up; whose existence is proved, not only by no liquors being, in health, collected in these cavities, but also by anatomical injections\*.

THESE

\* See *Kaumi perspiratio Hippocrati dicta*.



THESE absorbent veins, which, like those of the guts, have no valves, take up, by their attraction as capillary tubes, the rorid vapour of the arteries; after which, it is conveyed either to the valvulous lymphatics† or to the sanguiferous veins in which they terminate, by their vibrating motion, the pulsation of neighbouring arteries, and the alternate compression of the muscles. The absorption in the cavities of the *abdomen* and *thorax* is greatly promoted by the alternate pressure of the muscles concerned in respiration; while the muscles of voluntary motion employed in all kinds of exercise and labour, by accelerating the motion of the fluids in the absorbent vessels of the trunk and extremities of the body, enable them to imbibe more copiously. And hence we may see, why animals which move little, are generally oppressed with fat; while those which are kept at hard labour, are very lean. In the former, the absorbent veins of the fatty cells imbibe the oily matter

† Vid. al. *Monro jun. dissertat. de ven. lymphat. valvulos.*

ter deposited there very slowly, because they want the alternate pressure of the muscles of voluntary motion to push their contained fluid forward to the larger veins: In the latter, the absorption from those cells is not only increased by the various and continually repeated pressure of the acting muscles, but, the body being, by much exercise, in some measure exhausted of fluids, the veins imbibe more greedily, while the exhaling arteries pour forth their oily liquor more sparingly.

If the exhalant vessels of any cavity throw out too much, or if the absorbent power of the veins be weakened, or if both these happen together, a watery fluid will be collected in it; and in this way, are produced an *ascites*, *hydrocele*, *hydrops pectoris*, &c.

WHEN the blood is thin and watery and the vessels weak, anasarca, cedematous and other dropical swellings are common: for, as the bibulous veins can, by their attraction, only take up fluids in proportion to the depletion they suffer by means of their own

own vibratory contractions, and the alternate compression of neighbouring arteries and muscles; their absorbing power must necessarily be lessened in a lax state of the fibres, where those causes are much weakened.

FURTHER, while the redundance of a watery fluid in the blood increases the exhalation by the small arteries, it lessens the imbibition by the veins, for the same reason that ashes, sugar, or salts, when moistened, attract the watery particles of the air less strongly than when they are dry.

AGAIN, although there be little or no fault in the blood itself, yet, if its return from any part to the heart be much retarded, a dropsy of that part will soon follow; because the fluids taken up by the absorbents, will be slowly and not without difficulty received into the larger sanguiferous veins: and, as we have just now observed, their absorption must be in proportion to their depletion. Hence we see, why schirrous tumors, ligatures, and whatever compresses the veins, soon bring on dropical swellings.

IT also appears from what has been said, in what manner diuretics and purgatives carry off the stagnating waters in an *ascites* and other dropsies: for by the discharges they make by the kidneys and intestines, they not only lessen the quantity of watery fluid in the blood, but also, by their *stimulus*, increase the force of the circulation, whence the exhalation by the arteries must be lessened, at the same time that the imbibition by the veins is increased.

THE surface of the skin and vesicles of the the lungs are, like the other surfaces in the body, endowed with exhaling arteries and absorbent veins: by the former, there is perpetually discharged from the blood a fine lymphatic fluid; and, by the latter, the watery particles floating in the air are constantly conveyed into it.

WHEN the air is moist, and the body has been exhausted by fatigue, the imbibition by those veins often exceeds the exhalation by the arteries; as Drs *Keill* and *Linning* have



Have observed\*: but, taking the whole year round, the perspiration made by the skin and lungs exceeds their imbibition by about forty ounces a-day in *Great Britain*, and fifty-four ounces in *South Carolina*; which, though it has been commonly reckoned the total of the perspiration, is really no more than its excess above the quantity of fluid taken in by the absorbent veins of the skin, *fauces*, and lungs.

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ALTHO'

\* *Medicin. Stat. Britain. tab. iv. & observat. & Philosoph. Transact. No. 470.*

The remarkable imbibition by the skin observed by *Dr Linning*, July 3. 1740, betwixt 2½ and 5½ afternoon, happened, 'tis true, without any preceeding fatigue; but is easily accounted for, from his having, in that time, discharged 28½ ounces of urine: since so great a waste of the thinner parts of the blood must not only have diminished the exhalation by the cutaneous perspiring arteries, but also have increased the absorbent power of the imbibing veins every where through the body: and hence it is, that in a *diabetes* the urine often not only exceeds the quantity of liquors drank, but these are taken up so greedily by the absorbent vessels of the stomach and intestines, as to be discharged by the kidneys, before one would have thought they had got into the blood.

ALTHO' in vegetables, the vessels, which perspire in the heat of the day, frequently assume a contrary office in the night-season, and imbibe the dew and watery particles then floating in the air; yet it does not seem probable, that the exhaling or perspiring vessels of animals can thus become imbibing ones, or that the moisture of the air can be, by them, conveyed into the blood: since any motion in these vessels, from their extremities to their larger trunks, must be in opposition to the course of the arterial fluids.

THE imbibition by the vessels of the skin is performed in the same manner as in the other absorbents; only it is probable, that the perpetually varying oscillations of the external air may concur in promoting it.

ALTHO' the exhalations from animal, vegetable and mineral bodies, may be transmitted, along with the watery particles in the air, into the blood, by the absorbent veins of the skin and lungs, and thus account for pestilential and epidemical diseases raging at particular seasons; yet it is by no means probable, that *elastic* air can be imbibed by these

these vessels, and thus conveyed into the blood: for it has been observed, that air moves with great difficulty through capillary glass tubes, though some hundred times larger than the pores of the skin\*: and it is well known, that water and other fluids can penetrate many substances, through which air cannot pass.

THIS observation of the difficulty with which air moves through capillary tubes, may serve to determine a controversy which has long subsisted among physiologists, viz. Whether or not any *elastic* air enters into the blood by the lungs. For, since a few drops of water, with small portions of air between them, in a capillary tube, require a greater force to make them ascend, than that with which the tube attracts the particles of the water†; it must follow, that,

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if

\* *Aërem vero non nisi tardè et cum quadam tenacitate per hos tubos moveri, semper docuit experientia; aëri enim inest species quaedam tenacitatis aut immobilitatis. Muschenbroeck De tub. capill. vitr. cap. 1. exp. xi.*

† Muschenbroeck loc. citat:

if any *elastic* air were admitted into the absorbent veins of the lungs, it would not only not move through them itself, but would hinder their taking up, by their attraction, any other fluids.

THE prodigious swelling of animals in an exhausted receiver, further shews, that air cannot readily pass through the small pores of the skin and lungs. Nor is it any objection to this doctrine, that air has been found in the cavities of the heart; since, in a morbid state, this might arise from the blood, of which air is a constituent part, as well as of other fluids\*.

It is very observable, that air injected into the veins of an animal, produces obstructions, concretions in the blood and sudden death; which effects, however, may be easily accounted for, from the power which air has of coagulating blood, and from the surprising influence it has in stopping the motion of water, even in large pipes, especially when lodging in their flexures†.

BUT,

\* *Hales's Statical Essays*. vol. 1. chap. vi.

† *Philosoph. Transact.* No. 393.



BUT, to return ; as the effluvia of different substances floating in the air, are, by means of the cutaneous absorbents, conveyed into the blood, so likewise are the finer parts of plaisters, cataplasms, fomentations, and all other external applications: which ought therefore to be considered, not only as having a topical influence, but also as acting upon the whole body by their subtiler parts, which are mixed with the blood and other fluids.

It may be thought a difficulty, that quick-silver applied in the form of an ointment, should be taken in so readily by the absorbent vessels of the skin ; since, as has been observed above, it passes through the intestines without getting into the lacteals. But this happens from the particles of the mercury being extremely divided, and so united with those of the grease as to enter the pores of the skin along with them : for, though quick-silver is repelled by capillary glass tubes, yet, if their internal surface is run over

ver with melted greafe, it will be attracted by them\*.

We are told, that, upon opening the bodies of fuch as had taken mercury in large quantities, this fluid has been, fometimes, found in the cells of the bones and elfewhere †; the reason of which may be eafily underftood from what has been faid above: for, if the very fubtile and greatly divided particles of mercury after they are thrown out, by the exhaling arteries, along with the finer parts of the blood, into any cavity of the body, fhould unite by their ftrong mutual attraction, fo as to form globules whole diameters are larger than the diameters of the abforbent veins, 'tis evident, they could never be taken up by thefe veffels, but muft remain for ever in fuch cavity.

To conclude our obfervations on the abforbent veffels of animals; It may not be improper

\* *Memoires de l'academ. des sciences* an. 1724. and *Mufchenbroeck de tub. capill. cap. iv. exp. 12. cor. 2. and cap. vii.*

\* *Wepfer de apoplex. p. 277, and Mead on poifons, edit. 3.*

improper to take notice, that there are, upon the internal surfaces of the follicles and secretory and excretory ducts of the glands, bibulous veins, whose office is to carry off those fluids which would be improper to enter into the several secretions. And, if we suppose these absorbent vessels, like other capillary tubes, to attract, according to their different natures, different fluids more or less strongly, we shall see one great cause of the various secretions performed in the bodies of animals.

H. O. B.

happens as the notice, that there are  
upon the internal surface of the foli-  
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proper to carry into the genital functions.  
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like other capillary tubes, to be situated ac-  
cording to their different nature, distinct  
fluids more or less strongly, we shall see and  
great vessels of the various functions be-  
longing to the fluids of animals, &c.



## II.

# OBSERVATIONS

ON THE

**SENSIBILITY and IRRITABILITY**

OF THE

**Parts of MEN and other ANIMALS.**

Occasioned by *M. de Haller's* late Treatise  
on these Subjects.

*Spiritus intus alit ; Totamque infusa per artus  
Mens agitat molem——* VIRG.

The SECOND EDITION, corrected, and enlarged with  
Notes and an APPENDIX.

ADVERTISEMENT

*to the second Edition.*

M. de HALLER having, in the fourth volume of the *Memoires sur les parties sensibles et irritables*, given a particular answer to the first Edition of the following observations, I have in some of the notes to this edition, obviated such of that learned author's objections as appeared to be of less moment, reserving the principal points in debate between us, to be considered in the Appendix.

Edinburgh, June 16. 1761.

OBSERVATIONS  
ON THE  
SENSIBILITY AND IRRITABILITY  
OF THE  
Parts of MEN and other ANIMALS.

PART I.  
*Of Sensibility.*

THE truly learned and justly esteem-  
ed M. de Haller, in his late treatise *De*  
*partibus corporis humani sensibilibus et irrita-*  
*bilibus* \*, has favoured the world with an  
account of many new and curious experi-  
ments; from which he has frequently  
drawn such conclusions, as, if just, must  
necessarily produce considerable changes  
both in the theory and practice of the me-  
dical art. Being sensible how contrary his  
doctrine is, in many things, to the received  
I opinion

\* Acta Gottingenſ. vol. II. ad an. 1752, pag. 114.  
&c.

opinion of almost every physician, antient as well as modern, he has been at uncommon pains in making many and repeated experiments ; as much to overpower the incredulous by their number, as to secure himself from any chance of being deceived \*.

OPINIONS, *even* purely theoretical, should not be let pass, if there is any fallacy in them: but, when propositions, founded on experiments, and supported by men of high character, are advanced, by which practitioners in medicine may be led into errors ; it becomes the duty of every lover of the healing art, to prevent their being generally received as truths.

If the conclusions in the treatise above quoted, shall be thought just, physicians and surgeons will certainly treat their patients in a manner very different from what they have hitherto done ; whereby, if there be a mistake in the doctrine, many lives may be endangered or lost. It seems to be of some consequence, therefore, to consider  
this



this matter with attention, and to examine particularly, How far *M. de Haller's* system of sensibility is, or is not, well-founded.

## S E C T. I.

OUR author, in treating of the sensibility of the several parts of the human body, reckons, among the insensible parts, the tendons, *aponeuroses*, ligaments, *capsulae* of the articulations, *periosteum*, bones, marrow, *dura* and *pia mater*, *pleura*, *peritonæum*, *pericardium*, *mediastinum*, and *cornea*.

1. HE tells us, that living animals, whose tendons were cut, burnt, pricked, or torn, shewed no signs of uneasiness; and, when a little part of the *tendo Achilles* was left intire, they walked without any seeming pain\*.

2. WHEN the ligaments and *capsulae* of the articulations were pricked with a needle, scraped with a knife, or had oil of vitriol or *butyrum antimonii* applied to them,

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\* Aët. Gotting. vol. II. p. 120.

them, the animals shewed no sense of pain\*. The wounds of these parts and of the tendons were followed with no bad symptoms, and were cured without any other remedy than the *saliva* of the animal, and sometimes without this †.

3. The *periostrum*, when wounded, torn or burnt, caused no pain to the animals ‡.

4. He allows feeling to the teeth, but not to the other bones, because they are not furnished with nerves, and because he has seen the skull trepanned, without giving pain, in persons who were possessed of all their senses §.

5. He denies feeling to the marrow, not from any experiments of his own on living animals; but because it is a fatty substance and destitute of nerves §.

6. WHEN the *dura mater* was cut or lacerated, or burnt with oil of vitriol, spirit of nitre, and *butyrum antimonii*, the animal

\* Aët. Gotting. vol. II. p. 122. et 123.

† Ibid. p. 121. et 223. ‡ Ibid. p. 123.

§ Ibid. p. 124. § Ibid. p. 125.

animal seemed to have no feeling of the injury \*.

7. WHEN the *pia mater* was burnt by touching it with *butyrum antimonii*, the animals neither cried, nor were they convulsed; but, as soon as the brain itself was wounded, the body of the animal was twisted and distorted with violent convulsions †.

8. The *peritonæum*, *pleura*, and *pericardium*, when laid bare and cut, or otherwise irritated, produced no change in the animal ‡.

9. He denies feeling to the *mediastinum*, not upon the authority of any experiments, but because, like the *pleura*, it is a membrane and destitute of nerves ||.

10. HE reckons the *cornea* insensible, because its nerves cannot be demonstrated, and it is often pierced with a needle without giving pain §.

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BESIDES

\* Aët. Gotting. vol. II. p. 126.

† Ibid. p. 130.

‡ Ibid. p. 130.

|| Ibid. p. 131.

§ Ibid. p. 133.

BESIDES the insensible parts above mentioned, there are others which, according to *M. de Haller*, have either no sense of feeling, or a very obscure one; and these are the arteries, veins, glands, and *viscera*, viz. the lungs, liver, spleen and kidneys, which, when pricked, cut, or otherwise irritated, shewed nothing like feeling\*.

THE conclusions which our author draws from the above experiments, may be reduced to the three following.

1<sup>st</sup>, THAT the tendons, ligaments, *capsulae* of the joints, *dura mater*, *pleura*, and other membranes, are quite insensible.

2<sup>dly</sup>, FROM the insensibility of these parts, and the difficulty of tracing, by dissection, any nerves to them, he concludes that they have none, and that this is the reason why they are destitute of feeling.

3<sup>dly</sup>, HE thinks it follows, that those parts which, from his experiments, he concludes to be insensible, have been unjustly accused by physicians, as the seat of many painful diseases.

\* *Act. Gotting.* vol. II .p. 131 and 132.



diseases. Particularly, that the pain, swelling and inflammation which have often followed venæsection in the flexure of the arm, have not been owing to the tendons or *aponeuroses*, in that part, being pricked by the lancet, but to the median nerve or some branch of the musculo-cutaneous nerves being wounded\*.—That we need be no way afraid of wounds of the tendons, whether they be cut, pricked, burnt, or otherwise hurt.—That the *cephalæa* and *phrenitis* have not their seat in the *dura mater*.†——That the skin or subcutaneous nerves are the seat of the violent pain with which arthritic patients are affected, and not the ligaments or *capsule* of the joints‡. And that the pain of the pleurisy has been without reason supposed to be owing to an inflammation of the *pleura*, which is void of feeling. ||

IN

\* Act. Gotting. vol. II. p. 121.

† Ibid. p. 126.

‡ Ibid. vol. II. p. 122. and 123.

|| Ibid p. 130.

IN the few observations which I propose to make on this doctrine, I shall, *First*, Consider the parts, reckoned insensible by *M. de Haller*, in a sound natural state, such as they were in his experiments ; and *2dly*, When they are affected with diseases, whether in consequence of such experiments, or from other causes.

## SECTION II.

1. IN making or relating experiments, with a view to discover the sensibility or insensibility of the several parts of animals, particular regard should be had to an observation made by *Hippocrates*, above two thousand years ago, *viz.* That a greater pain destroys, in a considerable degree, the feeling of a lesser one\* ; an observation, the truth of which is confirmed by the daily experience of every physician. Thus, pricking any part of the body so as to give considerable pain, will so obliterate the irritation  
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\* Aphor. lib. 2. No. 46.

in the left orifice of the stomach, which is the cause of the hiccup, as instantly to put a stop to this convulsive motion. If a lighted candle be brought near a person whose eyes are a little inflamed, it will give him a good deal of uneasiness: but, if he be placed first in the sunshine, the candle will not add sensibly to his pain.

WHEN a frog's hinder-feet are pricked or otherwise wounded, immediately after cutting off its head, it makes scarce any motions at all with its legs, and shews almost no signs of feeling; but, if the toes are pricked or cut, ten or fifteen minutes after decollation, the legs and thighs are not only violently moved, but sometimes also the trunk of the body. Now, if in this case, as we see, the great pain occasioned by cutting off the head rendered the animal for some time insensible, when its toes were wounded, is it to be wondered at, that, after the more sensible parts were cut, those animals, which *M. de Haller* opened, shewed no signs of pain, when the less sensible parts were wounded?

WHEN

WHEN the *thorax* of a living animal is laid open, it does not seem to receive any additional pain by pricking or cutting its heart; no new convulsions are produced, nor any change in the body, except perhaps a quicker repetition of the heart's motion: does it follow from this, that the heart is destitute of feeling? No, surely; but only that, after the great tortures suffered by laying open the *thorax*, the new pain produced by wounding the heart is too small to make any remarkable impression upon a dying and half insensible animal.

Does it not appear, from what has been said, that a want of due attention to the above-mentioned maxim of *Hippocrates*, which is supported by the strongest experiments and observations, may have given occasion to some mistakes with regard to the degree of sensibility in many of the parts of animals? Thus, it will not follow, that the tendons, ligaments, *capsula* of the joints, *periosteum* and *dura mater*, are altogether destitute of feeling, because no convulsive motions or other signs of acute pain appear-

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ed in the animals when they were cut, pricked or torn ; for this might be owing either to their not being endowed with any painful feeling, or to the greater pain occasioned by cutting the skin, subcutaneous nerves, &c. in order to get at those parts, the sensibility of which our learned author proposed to try. The conclusion therefore which should be made from his experiments, is, not that the parts above-mentioned are wholly destitute of feeling, but that they are much less sensible than many others, or than has been commonly believed by physicians.

2. WITH regard to the marrow which *M. de Haller* reckons insensible ; *Duverney's* experiments made on men \*, (which have also succeeded with my ingenious friend and colleague *Dr Monro sen.*) and particularly his experiment made on a living animal before the

\* Dans les hôpitaux, où voyant panser ceux qui avoient eu un bras ou une jambe coupée, je pouvois voir la moëlle à decouvert,—toutes les fois que je la faisois toucher un peu rudement, les malade donnoit aussi-tot des marques d'une nouvelle douleur.

Memoires de l'Acad. des Sciences 1700. edit. 8vo, p. 255.

the Royal Academy of Sciences at *Paris*\*, are sufficient proof that this part is far from being destitute of feeling: and the reasons given by *M. de Haller* for his placing it among the

† “ Vous vous souviendrez, Messieurs, que je fis scier  
 “ devant vous, par le milieu, l’os de la cuisse d’un ani-  
 “ mal vivant ; et, ayant fait ôter les chairs et les mem-  
 “ branes pour laisser le bout de l’os entièrement à nud,  
 “ comme tous ces ebranlemens et ces divisions causoient  
 “ de douleur très cruelle à l’animal, j’eus la precau-  
 “ tion d’attendre que cette douleur fût passée, et,  
 “ quelque tems après, plongeant un fillet dans la mo-  
 “ èlle, vous vîtes que l’animal donna aussitôt des mar-  
 “ ques d’une très vive douleur, ce que fut reiteré plu-  
 “ sieurs fois avec la même précaution, et avec la même  
 “ succès.”

*Memoires de l’Academie Royale des Sciences 1700,*  
 edit. 8vo, p. 256.

*M. de Haller*, in answer to the authority of *Duverney* which I had cited, says, that “ a single experiment  
 “ is not sufficient to prove the sensibility of the mar-  
 “ row, which is evidently cellular, and whose nerves  
 “ have not yet been discovered.” \* But in confirma-  
 “ tion of the truth of *Duverney*’s experiments, I men-  
 “ tioned that they had also succeeded with Dr *Monro*  
*senior*; and have now to add that Dr *Al. Monro*  
*junior*, having been present last summer (1760) at the  
 amputation

\* *Memoires sur les parties sensible, vol. 4. p. 109.*

the insensible parts, are not of any weight, when compared with those experiments; for the feeling of the marrow is not owing to its oil, but to the membranes containing this oil; and the experiments which demonstrate its sensibility, prove that these membranes are furnished with nervous filaments, although they may be too subtile to be traced by the knife of the most accurate anatomist.

3. THE *tunica cornea* is so far from being insensible, as Dr *Haller* would persuade us, that any one may be soon convinced of the contrary by an easy experiment upon his own eye; for, when the *cornea* is touched with the point of one's finger, a very sen-

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amputation of the arm above the elbow, in a man of about forty, on account of a gun-shot wound in the hand and a supervenient mortification, after the arm was taken off, he pressed upon the bone, the marrow and the muscles repeatedly with the point of his finger; when the bone was touched the patient felt nothing, touching the muscles occasioned little pain, but he complained considerably, as often as the Doctor pressed the marrow with his finger.

sible pain is felt : and it is well known, that powder of tobacco, or any acrid liquor applied to the *cornea*, excites a very painful sensation. Though the sclerotic coat of the eye is far from being destitute of feeling, yet I have found it to be less sensible than the *cornea*, by touching both, not only with the point of my finger, but also with a bit of soft silk or linen\*.

## H A V I N G

\* *M. de Haller* remarks that in these experiments, I only touched the conjunctive (a) which is certainly true, nor was I ignorant of this, but as *M. de Haller* had pronounced the *cornea*, in general to be insensible, and had made no exception in favour of the conjunctive which covers it, my experiments were certainly in point, and the conclusion from them just : and it will be found very difficult to prove by any experiment, that the pain occasioned by cutting the *cornea* is not partly owing to this membrane, as well as to the conjunctive.

*M. de Haller*, unwilling to allow sensibility even to the conjunctive, ascribes the pain occasioned by touching the *cornea* to small branches of the fifth pair of nerves which run between these membranes. But supposing the existence of nervous branches between the *cornea* and conjunctive, as well as between this last and the sclero-

tic,

(a) *Memoires sur le parties sensibles*, vol. iv. p. 59 & 108.



HAVING had lately occasion to be present at the extraction of the crystalline *lens* in Mr *Sharp's* way\*, I enquired particularly at the patient, Whether he felt any pain when the *cornea* was first pierced with the knife employed in that operation: he told me, He thought the pain was much the same with what he used to feel when the skin of his arm was cut in blood-letting. It deserves however to be remarked, that,

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though

tic, although no anatomist has yet demonstrated the former. Yet the pain occasioned by touching the *cornea* very gently, or the sensation produced by the cool air blowing on it, cannot well be conceived to be owing to any thing else than the sensibility of its exterior covering. Nay, if the *cornea* itself were not more sensible than the sclerotic, why should the conjunctive feel more acutely where it covers the former, than where it is contiguous to the latter? The conjunctive where it covers the *cornea* is certainly one of the most sensible parts of the whole body, and least able to bear any hurt, or the application of any acrid substance. Nor could its sensibility be so great, if it were owing solely to some branches of nerves running between it and the *cornea*.

\* Philosoph. transact. vol. xlviii. p. 1. p 322.

though the skin and *cornea* are both endowed with a very considerable degree of sensibility ; yet, when they are cut quickly with a very sharp instrument, there is much less pain felt than one would imagine. Thus, when the skin is slightly wounded in shaving one's beard with a razor, the blood that follows is often the first thing that lets one know of any such thing having happened : and this, together with the pain occasioned by holding the eye firm in its orbit, and the concern the patients are generally in, may very well account for their being sometimes scarce sensible of any pain when the *cornea* is pierced with a sharp needle. But upon the whole, it appears, that the *cornea* is possessed of a remarkable degree of sensibility ; and consequently that *M. de Haller's* position, That all membranes are destitute of feeling\*, must admit, at least, of one exception.†

## 4. OUR

\* Act. Gotting. vol. 2. p. 130.

† *M. de Haller* represents me as inconsistent with myself in giving the *cornea* as an instance of a membrane that is sensible, after having owned that when pierced with

4. OUR author allows the kidneys either no feeling, or a very obscure degree of it ; because he could observe no signs of pain in the animals, whose kidneys he cut or pricked with a knife : but, after cutting the skin, abdominal muscles, &c. and displacing the intestines in order to get at the kidneys, it was scarcely to be expected, that the animals would shew any tokens of additional pain when these organs were wounded, unless they had been equally, or more, sensible, than the parts before dissected.

A physician of my acquaintance, who had occasion to see the operation of nephrotomy performed a few years since, was told by the patient that, when the kidney was opened, he felt pain, though duller and less acute than when the skin was cut.

One instance of this kind is more decisive in favour of the sensibility of the kid-

K 3 neys, with a sharp needle, it often occasions little pain\* ; but there is really no inconsistency here, since I have observed, at the same time, that the skin, which is among the most sensible parts, feels generally less from being slightly cut with a sharp razor, than the cornea does when pierced with a needle.

\* Vid. *Memoires sur les parties sensibles*, &c. vol. iv. p. 108.

neys, than twenty experiments on brutes, who cannot inform us whether they feel a slight pain or none at all; and, if the kidneys be less sensible than the skin, we cannot expect that wounding them will add considerably to the pain which the animals suffered before, from cutting the skin and muscles.

It is very observable that, while *M. de Haller* denies feeling to the kidneys, he allows it to the ureters: not because animals, when these are cut or wounded, shew signs of greater pain, than when the kidneys are treated in the same manner; but because he supposes the ureters to be of the nature of the skin, and propagated from it\*. And indeed, even, the strongest experiments upon brute animals would not have been sufficient to have proved the ureters insensible in men; when stones passing from the kidneys to the bladder, generally occasion such exquisite pain. But, does not the acute pain always attending a *nephritis*, and sometimes occasioned by a stone lodged in the kidneys, shew, beyond doubt, that they are endowed with

\* *Act. Gotting. vol. 2. p. 131.*



with feeling, as well as the ureters? while nothing can be concluded from *calculi* lying long in the kidneys without giving pain\*, except that they were so situated as not to hurt them.

5. ALTHO' brute animals shew small signs of sensibility, when the glands are pricked, or have acrid things applied to them, immediately after the very sensible skin has been cut; yet, we know that a bruise on the testicles often causes, instantly, such exquisite torture, as to make men faint; and a blow on a woman's breast often excites, immediately, shooting pains in the gland there, though no mark of the bruise appears in the skin. These are such undoubted proofs of the sensibility of the glands, as no experiments made on brute animals will ever be able to overthrow.

6. *M. de Haller* allows the membranes of the *aorta* near the heart, and of the temporal, lingual, labial, thyroid, and pharyngean arteries, to be sensible; but thinks the coats of the arteries in other parts of the  
body

\* *Act Gotting.* vol. 2. p. 132.

body have either no feeling, or a very obscure degree of it; though it does not appear from his experiments, that animals complained more when the former, than when the latter, were irritated. In this case, he relinquishes the appeal to experiment, and founds his opinion on his tracing nerves to the former, which he could not do to the latter: an argument he makes use of upon several other occasions, and which is next to be examined.

7. As our author not only founds his opinion of the insensibility of many parts of the body upon experiments made on living animals, but, also, on their being destitute of nerves; we shall briefly consider, whether, from the real or seeming insensibility of any part, or from anatomists being unable to demonstrate its nerves, we are intitled to conclude that it has none.

ALTHO' the tendons are quite insensible according to *M. de Haller*, and their nerves can scarcely be demonstrated by anatomists; yet, we are convinced, that the tendons are not destitute of nerves, from the following

ing obvious observation. In foetuses and new-born children, many parts which, afterwards, in an adult state, become tendinous, are muscular, or partly so; and, as animals advance in age, the proportion of the tendinous, to the muscular part, gradually increases: we must either, therefore, deny nerves to the muscles, or allow them to the tendons also\*.

ALTHO' we cannot trace nervous filaments to the small arteries, we have reason to believe they are furnished with them; else, how could the distraction of their coats in inflammations, occasion such acute pain†?

\* The reader will observe that from those parts which were once muscular, becoming afterwards tendinous by age, I only conclude that they are not destitute of nerves, but not that they are sensible; which however, *M. de Haller* has represented me as saying,\* although it be expressly contrary to my acknowledgment of the very obtuse feeling of the tendons, and to what I have said below, of the sensibility of the parts, depending not solely on their having nerves, but on the disposition and state of these nerves.

† *M. de Haller* seems sometimes to allow sensibility

\* *Memoires sur les parties sensibles*, vol. iv. p. 103.

I think we may conclude every part that is liable to be inflamed by irritation, to be, in some degree, sensible and endowed with nerves; for, since the inflammation cannot in this case be owing to any increased force of the heart, the distension of the small arteries, and the greater *impetus* of the blood in them, must be owing to an increased oscillatory motion in the vessels themselves, excited by the unusual irritation: but, these motions of the small vessels being of a like kind with those alternate contractions which are observed in muscles whose fibres have been irritated, it will follow that those vessels partake of a muscular nature, and consequently have nerves like the other muscles.

WITH regard to the membranes; since the *dura mater* and *pleura* are furnished with nervous filaments, which anatomists have been to the arteries and at other times denies that they have any, because when they are tied the animals do not seem to feel any pain; but it is to be observed, that the ureters, whose sensibility he acknowledges, may be tied or wounded with as little signs of pain as the arteries. Vid. *Memoires sur les parties sensibles*, vol. iv. p. 87. & 110. and *Art. Gotting.* vol. 2. p. 142.



been able to demonstrate \*, we may reasonably conclude that the other membranes are not destitute of them; altho' they may be too small to come under the eye of the best dissector: this is certainly true of the *cornea* and membranes containing the marrow, which we have shewn, from undoubted experiments, to be sensible, and consequently not without nerves. It appears therefore, that we can by no means conclude any part to be insensible, *merely* because its nerves cannot be demonstrated.

ON the other hand, it is allowed, that we cannot certainly conclude, from a part's being furnished with nerves, that it is sensible at all, or in what degree: for, the nerves must be in a certain state to perform their offices rightly; and, in proportion as they recede from this, their sensibility will be more or less blunted. Examples will illustrate this.

THE bones, which in a natural sound state are insensible, are nevertheless most certainly

\* Winslow exposition. anatom. sect. ix. No. 35. and sect. x. No. 47.

ly furnished with nerves; as appears from the remarkable sensibility of the granulated substance which rises from them after fractures, or their being chizelled, or when they exfoliate: this soft flesh, however, gradually lose its feeling as it grows harder, till being, at last, turned into a callous or osseous substance, it becomes wholly insensible.

THE membranes of the *tela cellularis* are, in a natural state, soft, flexible and distensible, and have little feeling; but, in every wound or ulcer, when they acquire some more firmness, they are sensible of every touch and every acrid application, as surgeons see daily. After a cicatrice has, sometime, covered the parts where the sore was, and they have returned to their natural softness, these cellular membranes lose again their sensibility, as appears on making a new wound thro' the cicatrice; and recover it again, whenever they become firm and tense, by the new inflammation and suppuration.

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THE *dura mater*, which, in a sound state, has but little feeling, granulates after the trepan, and feels every irritating substance applied to it; and the same thing happens to cartilages, ligaments, tendons, membranes, &c.

WITHOUT attention to this change in the firmness of parts, and its effect upon their nerves, we could never account for what has been observed above, viz. that the parts of muscles, which in foetuses and children are lax contracting fibres and very sensible, become, in a great measure, insensible, in a sound state, when, by the creature's advancing in age, they are compacted into tendons, as happens to many of them.

If sensibility, then, be a sure mark of the existence of nerves in any part of the body, there is not one that is destitute of them, altho' anatomists will never be able to demonstrate them in every part.

FROM what has been said, it may appear, that *M. de Haller's* experiments on living animals do not sufficiently prove the doc-

trine he would deduce from them; and that his argument for the insensibility of parts, taken from their nerves not being demonstrable, is altogether inconclusive. Let us next try what further light diseases will throw upon this subject.

### S E C T. III.

IF the parts reckoned insensible by *M. de Haller* were really destitute of nerves, it would follow that they could in no case become the seat of painful sensation; and even supposing them furnished with nerves, but possessed only of an obscure degree of feeling, it may be thought, at least, not probable that they can be the seat of those painful diseases commonly ascribed to them. In order to set this matter in a proper light, it will be sufficient to distinguish between parts in a sound and in a diseased state. In a sound state, the feeling of many parts of the body is but very dull, which is altogether necessary to prevent the uneasiness



we would otherways perpetually suffer, when our organs are stretched, pressed upon, &c. in the common offices of life: such parts, therefore, when cut or wounded, in a sound state, give little uneasiness; but, if afterwards an inflammation comes on them, they become extremely sensible, and their over-stretched vessels and nervous filaments occasion intense pain, by which we are excited to endeavour the cure of the disease.

It is certain, that those parts which are most sensible in a sound state, acquire a more acute feeling when inflamed. Thus the stomach, which, in health, can bear the touch of wine, brandy, and other pungent liquors, without being hurt, is, when inflamed, often brought into convulsions by the mildest drinks; and light, which gives no sensible pain to the eye in a sound state, becomes intolerable when this organ is inflamed. Nor can we doubt that the more insensible parts may acquire, when inflamed or otherways diseased, a remarkable degree of sensibility. Examples above re-

cited have shewn this to be true of the bones, *tela cellularis*, and *dura mater*; and the following facts will shew the same thing, in other parts, reckoned either wholly, or almost wholly insensible by our author.

As often as there is an inflammation, especially when tending to suppuration, in any of the glands, as the parotids, tonsils, maxillaries, *mammæ*, *testes*, kidneys, &c. the patient is tortured with pain, often, before the teguments are affected or even considerably stretched. And is not this a much better proof of the sensibility of these parts, than *schirri* and other indolent swellings are of the contrary?

THE fore-part of the eye, when inflamed, can bear the touch of no hard or acrid substance; and *fungi* rising from it, give very sharp pain, when fretted.

IN the rheumatism, joints, where the skin is unstretched and of the natural colour, and where no muscular fibres are placed, are severely pained on the least motion, tho' done without the effort of the patient, which

which must therefore depend on the sensible ligaments and tendons; since large branches of nerves, thus affected, would produce convulsions of the muscles they serve, which do not happen: besides, in these cases, the pain is not felt where the large nerves are.

A contusion, by a fall on the great *trochanter* of the thigh, without causing *ecchymosis*, or swelling of the teguments, often brings, in a little time, racking pain on all the outside of the thigh, leg and foot; which continues obstinately for months or years thro' the whole extent of the *fascia lata*.

AN inflammation of the *periosteum*, as in the *panaris*, where the suppuration happens between this membrane and the bone, nay, even the repletion of the vessels of an over-stretched *periosteum*, as by heat or food in venereal nodes, gives very sharp pain. And, in the *spina ventosa* and other suppurations of the marrow, pain is felt before any signs of the disease appear externally.

THESE observations seem to demonstrate, beyond doubt, that many of those parts, which M. *de Haller* would have us believe to be insensible, are often the seat of remarkable pain in the human body; and, I cannot help thinking, that, in other examples, where he endeavours to assign a different seat of the painful sensation, he is mistaken, and is laying the foundation of dangerous practice. It will, therefore, be worth while to examine these cases.

1. HE imagines that the pain, swelling, and inflammation of the arm, which have sometimes followed the opening of the *median* vein, must have proceeded, not from a wound of the tendon of the *biceps* muscle, but of the *median* or some other nerve. But, if this were the case, why should not similar symptoms sometimes follow bleeding in the cephalic or jugular veins? In opening the jugular vein, some nervous filaments are frequently wounded, and often occasion a sharp pain, as if the point of the lancet had been left in the wound; this, however, goes off in a day or two, or sooner



fooner, without leaving any bad consequence. But the mischiefs which have followed bleeding in the *median* vein are of a different kind; tho' little or no pain is felt at first, yet afterwards, not only the whole arm is violently pained and swelled, but a particular hard swelling is often formed in the place where the wound was made, from which a thin lymph issues; and the patient does not recover the full use of his arm for several months; nay, sometimes loses the the motion of the elbow-joint altogether. And, that a wound in the tendon is, at least, sometimes the cause of those symptoms that follow blood-letting in the flexure of the arm, appeared evidently in a patient who died in this place, some years ago, of a fever occasioned by the pain, swelling and inflammation, consequent upon opening the median vein of the right arm, the *tendo bicipitis* of which was swelled to near ten times its natural bulk.

How very sensible tendons may become, when inflamed, appears from various observations;

servations ; particularly one mentioned by the Baron *Van Swieten*, of a Nobleman, who was seized with most terrible convulsions over his whole body the moment his surgeon took hold of one of the tendons near his ankle, mistaking it for a part of the fatty membrane \*.

2. OUR author ascribes the pain of the gout to the skin or subcutaneous nerves, and not to the *capsula* or ligaments of the joints affected. But does not the rigidity of the joints, which the gout at last produces, shew, that its seat is deeper than the skin or nerves below it ; and that the ligaments of the articulations, and tendons of the muscles which serve for their motions, are affected ?

WHEN one sprains his wrist or ankle, there is often no great pain felt immediately ; but soon after, when the overstretched parts begin to swell and inflame, a considerable pain ensues, which is greatly increased if the joint be moved. Does not the pain in this case proceed chiefly from the overstretched ligaments  
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\* Comment. in Aphor. Boerhaave, vol. II, p. 241.

or tendons? It will be hard to persuade physicians, that it is owing to any hurt received by the skin or subcutaneous nerves. And, if the ligaments or tendons may be affected with pain from being too much stretched, why may not they be the principal seat of that pain which affects the joints of gouty patients?

CHALK-STONES in a joint frequently give sharp pain before they pierce the capsular ligament, and before the skin is much stretched or red. Further, without allowing sensibility to the ligaments, let any one try to explain what my learned colleague Dr *Monro senior*, and, I dare say, many others, have oftener than once seen in practice. A pea-issue, for a dropsy of the knee, put in with a caustic or a knife, and dressed with the pea a considerable time, created little uneasiness to the patient; but, after a puncture of a lancet, made, very near to where the issue was, through the *capsula* of the joint to let out the water, most racking pain and inflammation.

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inflammation ensued, which brought the patient to the brink of the grave.\*

3. OUR author is of opinion, that the the insensible *dura mater* cannot be the seat of a headach or *phrenitis*. But how little sensible soever this membrane may be in a natural state, yet, if it may be affected with pain, as often as it is inflamed or obstructed, it may still be, in many cases, the seat of those diseases. In patients who have died of a *phrenitis*,

\* In answer to this, *M. de Haller* has thought proper to remark, that *Mr Warner*, who is much better acquainted with surgery than *Dr Whytt*, recommends the opening the capsular ligament as the only effectual cure in a dropsy of the articulations\*. The impropriety of this remark must appear in a strong light to the reader, when he observes that what I assert here, is *solely* upon the authority of Doctor *Monro senior*, whose skill in surgery, as well as his accuracy in making observations, are too well known to be called in question.

But further, the pains which followed the opening of the capsular ligament in the only case which is mentioned by *Mr Warner*, though they were less violent than is usual in such cases †, shew that this ligament, however little feeling it has in a sound state, may, when diseased, become the seat of painful sensation.

\* *Memoires sur les sensibiles*, vol. 4. p. 39.

† *Vid. Philos. Transact.* vol. xlix. p. 457.



*phrenitis*, the *dura* and *pia mater*, as well as the cortical substance of the brain, have been found inflamed, suppurated, and mortified: and in those who, after recovering once and again of a *phrenitis*, have died of other diseases, the *dura* and *pia mater* have been found much thicker and harder than usual \*.

As the headach generally attending fevers often begins several days before any signs of a *delirium* appear, we cannot ascribe it to an obstruction in the cortical part of the brain, but in the *dura* or *pia mater*. Nor can this headach have its seat in the exterior teguments of the skull; otherways, the pain would be increased by pressing the part chiefly affected, as often happens in those periodical headachs which seem to have their seat in the subcutaneous nerves, or *pericranium*.

LASTLY, *M. de Haller* thinks, that the intercostal muscles, or large nerves running between the ribs, are the seat of the pain of the pleurisy, and not the *pleura* itself, which

\* Van Swieten comment. in Boer. aphor. vol. 2. p. 604.

which is insensible. But, if this membrane, notwithstanding its small degree of sensibility in a sound state, may be affected with great pain, when inflamed; it will hardly be doubted that it is sometimes the seat of the pleurisy: since, in patients who have died of this disease, the *pleura* has been found inflamed and suppurated\*.

BUT, besides the insensibility of the *pleura*, *M. de Haller* has brought another very plausible argument to prove, that the pleurisy can never have its seat in this membrane; *viz.* the patient's feeling the greatest pain in inspiration when the ribs are brought nearer each other, and consequently when the *pleura* is less upon the stretch than it was in time of expiration. But this learned author has long ago very justly observed, that ordinary and gentle inspiration in men, is chiefly performed by the diaphragm, while the intercostal muscles are scarce employed at all†: wherefore in inspiration, which pleuritic patients perform

\* Van Swieten comment. vol. 3. p. 8.

† Praelect. in institut. med. Boerhaav. vol. iv. No. 615. not. (a).

perform with great caution, the ribs may be supposed to alter their situation very little†; but, as the inferior part of the *pleura* must be somewhat stretched by the descent of the diaphragm in inspiration, it is no wonder the pain should be then most acute.

In women, especially such as are pregnant, who use the intercostal muscles more in ordinary inspiration than men, the *pleura* will be more stretched at that time than during expiration; because the cavity of the *thorax* is increased in wideness and depth, as well as length.

With regard to what *M. de Haller* says of the ribs approaching each other in inspiration; though this is certainly true of the superior

† It is somewhat surprising, that *M. de Haller* should have mentioned the approach of the ribs in inspiration, as an argument to prove, that the pleurisy never has its seat in the *pleura*; after having formerly told us, that pleuritic patients don't use the intercostal muscles at all, but breathe by means of the diaphragm alone. *Praelect. in institut. med. Boerhaav. vol. iv. No. 615. not. (a), and No. 619. not. (c).*

superior ribs, yet I have some doubt, whether it be so in the inferior ones: for, in a very full inspiration, I can with my fingers plainly feel the six or seven inferior ribs recede from each other, and approach again in the succeeding expiration \*. Wherefore it must appear, that the increase of the pleuritic pain in time of inspiration can be no proof, that the disease has not its seat sometimes in the *pleura*.

UPON the whole, although *M. de Haller's* experiments clearly shew, that several parts of animals are possessed of a much more obscure degree of feeling than has been commonly imagined; yet it is hoped, the reader will, after weighing what has been said, be far from pronouncing them altogether insensible, or condemning the uniform opinion

\* The reason why not only the false ribs, but also some of the true ones, rather recede from than approach each other in inspiration, may be understood from what is briefly said concerning the motions of the *thorax*, by Dr *Monro*, in his anatomy of the bones, edit. 5. p. 242.



opinion of physicians in all ages, concerning the parts which are affected in many diseases; and, instead of it, embracing a doctrine which is far from being sufficiently proved, and may, if made a foundation for practice, be of fatal consequence.

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PART

## PART II.

### *Of Irritability.*

#### SECT. I.

**A**LTHO' many of the parts composing the human body are endowed with a considerable degree of elasticity, whereby they restore themselves when overstretched; yet muscular fibres alone are possessed of a proper contractile power, which is exerted, in consequence either of an effort of the will, or of some *stimulus* applied to them, or their nerves: by the former, voluntary motion is produced; by the latter, involuntary\*. The learned *M. de Haller*, who chuses to call the contractile power of irritated muscles by the name of *Irritability*, has, by a variety of curious experiments upon living animals, shewn, that it is a property of all muscular fibres; and that no part, which is not muscular, is irritable, although, of the muscular

\* Vid. an Essay on the vital motions, &c. sect. i. art. 2.

lar parts, some are more, and others less sensible of irritation. But when, in his enumeration of the parts of the body that are or are not irritable, he allows irritability to the lacteal veins, mucous glands and sinuses, and yet denies it wholly to the kidneys and ureters, and almost wholly to the arteries, veins, and excretory ducts of the glands, we cannot help differing greatly from him: since these last parts are, at least, as much muscular as the former; and since our learned author's experiments on living and dying animals, shew neither the one nor the other to be irritable\*.

THAT the small arteries are not destitute of irritability, may be demonstrated by undoubted experiments. Thus, when an acrid cataplasm is applied to the skin, or spirit of wine to the eye, whence proceeds the inflammation which is soon produced in the skin and almost instantly in the eye? Not, surely, from any increased force of the heart or larger arteries, but from the irritated vessels them-

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selves,

\* Act. Gottingenf. vol. 2. p. 139—143.

selves, which are agitated with strong alternate vibratory contractions; by means of which the moment of the blood in them is greatly increased, and red globules are pushed into those vessels which, in a sound state, only receive serum or lymph\*.

NOR

\* *M. de Haller* says that the irritation of the small arteries, if they are hollow muscles, ought rather to empty them, than increase their diameter (a), but a little attention will show this opinion to be ill founded. If an irritated artery could empty itself as easily as the bladder of urine, and were as slowly supplied with new fluids, an irritation of it would have the effect mentioned by my learned adversary, but since the arteries are furnished with a continued supply of blood from the heart, it is easy to see, that as often as by any considerable stimulus, the alternate contractions of the small arteries of any part are greatly increased, the force of the blood must not only be augmented, but these arteries, as well as the smaller lateral branches which they send off, must be enlarged in their diameters, and contain a greater quantity of fluids than usual, i. e. the part will be inflamed.

The increased heat, redness and pulsation in an inflamed part can only be conceived to happen, either from

(a) *Memoires sur les parties sensibles*, &c. vol. iv. p. 113. & 114.



NOR can we conclude that the arteries are destitute of irritability, because the *aorta* was not observed to contract itself when pricked with a sharp instrument, or touched with acrid liquors\* ; since the same is true of the mucous glands and sinuses, which yet our author allows to be irritable†. And it is not improbable, that the small capillary arteries may be more irritable than the *aorta* or larger ones ; because their muscular coat

from an increased force of the heart and larger arteries, or of the small vessels themselves, but we know that in many topical inflammations from external causes, the force of the heart and large arteries is not altered, wherefore the inflammation must in such cases be ascribed to the increased alternate motion of the small vessels themselves ; for their continued spasmodic contraction would occasion a sense of cold and a paleness, not a heat and redness.

*Lastly*, Since an irritation of the salivary and lachrymal vessels, and of the mucous ducts and sinuses increases the motion of the fluids in them, it will be difficult to give a reason why it should not have a similar effect in the small arteries every where through the body.

\* *Act. Gottingenf.* vol. 2. p. 141.

† *Id.* p. 143.

coat, as it is called, is much less firm and tendinous.

FARTHER, *M. de Haller* reckons the lacteal veins irritable, because, after death, they contract themselves so as to expel the chyle and become invisible\*; but do not all the arteries of the body, small as well as great, also contract themselves after death, and push most of their blood forward into the veins? And is not this coarctation of the lacteals owing more to the elasticity of their coats now increased by cold, than to a proper muscular contraction? However, if the lacteals be irritable, as is, I think, very probable, though for other reasons than the one now mentioned; it will follow, that the lymphatic and other vessels of the body are so likewise: for the lacteals are only a kind of lymphatic veins arising from the villous coat of the guts, which, on account of the colour of their fluid, have got the name of *lacteal*. Nor have we any reason, from their muscular structure, to ascribe irritability to the lacteals and thoracic duct,

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\* *Act. Gotting. vol. 2. p. 142*

more than to the other vessels of the body.

WITH regard to the veins, I shall only observe, that, since the alternate contractions of the trunks of the *vena cava* near the heart, shew them to be possessed of a remarkable degree of irritability ; it is not probable that the other veins are *wholly* destitute of it. I know that *M. de Haller* denies any proper motion to the *cava*, and ascribes its seeming alternate dilatation to the blood pushed back into it by the contracting auricle\*. But, if this were true, how could the *cava* contract five or six times before the right auricle performed so much as one pulsation, as *Steno* has observed in rabbits† ? or how could it possibly continue its alternate motions, not only for a considerable time after the right auricle had ceased to move‡, but even after the heart, together with this auricle, was intirely separated from it ?

\* *Primæ linæ physiolog.* 2. edit. No. cxiii.

† *Bartholin. epist. med. cent. iv. p. iii.*

‡ *Bartholin. epist. cent. iv. p. 110. and Essay on vital and involuntary motions, p. 354.*

it\*? These facts shew so clearly that the motions of the *vena cava* do not proceed from the alternate contractions of the right auricle, as to make any further observations on our learned author's mistake, as to this matter, altogether needless†.

DOES not the sudden flow of pale urine in hysteric cases, and the increased derivation of *saliva* into the mouth of a hungry person from the taste or even the sight of grateful food, shew that the secretory vessels of the kidneys and excretory ducts of the salivary glands are, in such cases, agitated with an unusual oscillatory motion, and consequently not destitute of irritability? Nor ought *M. de Haller* to have denied this power to the vessels of the kidneys and excretory ducts of the glands: since he allows it

\* Walæus de motu sang. ad. fin. anatom. Bartholin. p. 783.

† *M. de Haller* in his later writings, acknowledges the motions of the *vena cava* to be owing to its own fibres; and has further shewn that the pulmonary veins near the left ventricle of the heart are endowed with the



it to the lachrymal glands and mucous sinuses, because they pour forth their fluids more copiously when stimulated; although his experiments discovered no signs of irritability in them\*.

WHEN a stone passes from the kidneys to the bladder, does not the irritation of the sharp stone occasion some kind of spasmodic contraction in the *ureter*: and does not a large dose of *opium* facilitate its passage, by abating or destroying the painful feeling, and consequently lessening the constriction of the *ureter*? This canal, therefore, seems to be possessed of some kind of irritability, notwithstanding *M. de Haller* tells us, it was, in the animals

the same power: But from his remarks on this passage, the reader would imagine that *M. de Haller* had never ascribed the motion of the *vena cava* to its dilatation by the blood pushed back into it by the right auricle, and that I had charged him falsely with this opinion†; which nevertheless is to be found in No. cxliii. of the second edition of his *prima Linea physiolog.*

\* Act. Gotting. vol. 2. p. 143.

† *Memoires sur les parties sensibles*, vol. iv. p. 116. 117.

144 *Observations on Irritability.*

animals he opened, insensible of the *stimulus* of oil of vitriol \*†.

If our author's experiments discovered no kind of irritability in the blood-vessels, lacteals, glands and mucous sinuses, it will not follow that the *iris* is destitute of this power, although it did not appear to contract when irritated with a knife †.

THE DOCTOR adds, that the dilatation of the pupil cannot be owing to any muscular power because it becomes widest at death or immediately after it ||. I have elsewhere observed that the dilatation of the pupil is owing to the longitudinal fibres of the  
*uvea*,

\* Gotting. Aët. vol. 2. p. 142.

† As a further proof of this, my ingenious Colleague Dr *Alexander Monro junr.* informs me, that in a pig which was strangulated and half dead, he observed the *ureter* to contract very remarkably, when he touched it with the point of his finger; nay, when he moved the point of his finger along the surface of the ureter, a successive contraction of this canal was produced from above downwards.

‡ Aët. Gotting. vol. 2. p. 143.

|| Ibid.

*uvea*, which by their natural contractility retract its edges, when the orbicular muscle is not excited into contraction by the action of light on the *retina* \* : at death, therefore, when the eye becomes insensible, the pupil must be very wide ; but, sometime after death, as the accurate *Winflow* has always observed †, and I have also seen, the pupil becomes narrower, because the longitudinal fibres of the *uvea* lose their tone, become flabby, and are elongated‡. Nor does *M. de Haller* seem to have attended to what is

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\* Essay on vital motions, sect. vii.

† *Memoires acad. des sciences* 1721. edit. 8vo. p. 416.

‡ *M. de Haller* mentions in several of his experiments that he observed the pupil very wide in animals not only at the time of death, but for some hours after it : Nor is this inconsistent with *Winflow's* having almost constantly found the pupil of a moderate size in the human body a day or two or perhaps longer after death ; for the pupil, which continues wide for several hours after death in men, as well as in other animals, becomes narrower as soon as the fibres of the *uvea* have lost their tension and elasticity : Nay, I have observed the pupil, after having been remarkably wide and destitute of all motion for some time, become narrow even a day

said in page 111, and 129. of my essay on the vital motions, &c. when he mentions the dilatation of the pupil at death, as a clear proof that it is not owing to the contractile power of the fibres of the *uvea*; since this very dilatation of the pupil, compared with its coarctation some time after death, demonstrates the truth of what I have advanced. But, after all, if the dilatation of the pupil be not owing to the elasticity or natural contractility of the radiated fibres of the *uvea*, To what cause can it be ascribed? For 'tis presumed, our author has given up his notion of the aqueous humour pressing the edges of the pupil outwards, as being contrary to the known laws of hydrostatics. It may not  
day or two before death in two boys who died of a drop-sy in the ventricles of the brain. In these patients, sp. of sal. Ammon. held to the nose, or a spoonful of a cordial julep, made the pupil instantly as wide as it is observed to be in a confirmed *gutta serena*; but soon after, it became narrower again; which is a sufficient proof that the dilatation of the pupil, in this case, was owing to a contractile power communicated to the fibres of the *uvea*, by the *stimulus* of the volatile spirits or cordial; while its coarctation, afterwards, was only the consequence of the *uvea* being more relaxed.



not, however, be improper to observe here, that, although we should suppose the *uvea* to be, strictly speaking, not muscular, but, only a cellular membrane; yet, like the *dartos* of the *scrotum*, it would, by its elasticity, retract the edges of the pupil as soon as the cause contracting it ceased to act. And although, at the time of death, the pupil would, hence, be rendered very large, yet sometime after it, when this cellular substance began to lose its elastic power, the pupil would become narrower.

*M. de Haller*, because he cannot discover any orbicular muscle surrounding the edge of the pupil, concludes there is none; and ascribes the contraction of this part to a stronger influx of fluids into the small vessels of the *uvea*, occasioned by the *stimulus* of light acting upon it. The insufficiency of this hypothesis we have shewn elsewhere;\* and shall only add, that, as we conclude from the various motions of many of the smaller insects, that they are, as well as larger animals, endowed with muscles, though

\* Essay on vital motions, p. 127. &c.

we can neither demonstrate these instruments of motion by the anatomical knife, nor by the assistance of the microscope, so we may infer the existence of the orbicular muscle of the *uvea* from the regular motions of the pupil, although its texture may be so delicate, as scarcely to be distinguished by the anatomist from a dense cellular membrane.

BUT, to return; there are some other things advanced by our learned author, in his account of the irritable parts of the body, which, though not satisfactory, we shall pass over\*; and proceed to consider what

\* *M. de Haller* has represented me as saying, That the contraction of every muscle of the body is interrupted with alternate relaxations (a): whereas in p. 20, 257, 260 and 261, of my Essay on the vital motions, I have expressly excepted the *sphincter pupille*, muscles of the internal ear, and some others, whose contraction is owing to a *stimulus* acting on some neighbouring or distant part. I have indeed affirmed, That those muscles, to whose fibres a *stimulus* is immediately applied, are always agitated with alternate contractions and relaxations; and *M. de Haller* himself agrees with me in this, when he says, that all muscles, not so much

(a) *Art. Cutting*, vol. 2. p. 145.

what he has offered concerning the nature of irritability.

## S E C T. II.

IN my Essay on the vital and involuntary motions of animals, I had endeavoured to shew, that *stimuli* applied to the muscles of animals excited them into contraction, by producing an uneasy feeling in them or their nerves; but *M. de Haller*, who thinks irritability

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as one excepted, that he knows of, tremble and palpitate after death, and are alternately contracted and relaxed (*Act. Gotting. vol. 2. p. 139, & 144*). The bladder of urine, however, seems to differ from the other muscles or muscular organs in contracting uniformly and without any alternate relaxations, when it is pricked or exposed to the cold air, in animals that are dying or newly dead\*. In this respect the bladder resembles the *dartos scroti*, which is excited into a continued contraction, and furls up the *scrotum*, when cold water and astringent or acrid liquors are applied to it, or when the skin of the scrotum is gently irritated by titillation.

\* *Act. Gotting. vol. 2. p. 142 & 145.*

lity an innate property of muscular fibres, contends, that it does not depend upon the nerves, and has no connection with sensibility.

1. BECAUSE the most sensible parts, such as the nerves and skin, are not irritable \*.

2. BECAUSE the irritability of our organs is observed to be in proportion to their sensibility †. And,

3. BECAUSE parts destitute of feeling are irritable ‡.

WITH regard to the first of these, since muscles are the only organs of the body, which, by their particular fabric, are fitted for motion, it is so far from being a wonderfull discovery, as our author seems to think, That the nerves are destitute of irritability, that it is only a necessary consequence of their make; for a power of contraction does not depend on sensibility alone, but upon this in conjunction with a particular structure,

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\* Act. Gotting. vol. ii. p. 134.

† Ibid. p. 136.      ‡ Ibid. p. 134.



THE proper answer therefore to this first argument, is, That, altho' irritability always infers some degree of sensibility, yet sensibility does not infer irritability, unless the part be, by its peculiar fabric, fitted for motion, *i. e.* in other words, unless it be what we call muscular \*.

ALTHO' the skin is not irritable in the same sense that the muscles are, yet the inflammation and pain raised in it by blisters and other acrid applications shew, that it is very readily fretted or irritated by *stimuli*. The skin, when stimulated, is not brought into alternate contractions, because it is not by its structure made capable of this

\* Notwithstanding what is said here to shew that sensibility does not infer irritability unless in parts which are muscular, *M. de Haller* has accused me as being guilty of a gross inconsistency, when I say irritability is proportional to sensibility, because I acknowledge the nerves to be destitute of this power. See *Memoires sur les parties sensibles*, &c. vol. iv. p. 118. There is no inconsistency in what I have advanced on this head; and surely nothing but inadvertency could have been the occasion of *M. de Haller's* misrepresenting me in the manner he has done.

this kind of motion ; but it becomes red, is inflamed, and pours forth its liquors so copiously, as to separate the scarfskin, and raise it in the form of a bladder filled with water, because the small vessels, of which it is in a great measure composed, partake of a muscular nature, and are, like the larger muscles, excited into alternate contractions by *stimuli*.

FURTHER, the *dartos* or cellular membrane of the *scrotum* is contracted uniformly, when exposed to the cold air or other *stimuli* ; and the skin, from the application of cold air or water, seems likewise to suffer some kind of contraction, by which means it is raised into tubercles resembling the skin of a goose. When cold water is suddenly, and without one's knowledge, applied to a part of the body that is warm, there is excited instantly a kind of shivering which spreads over the whole body ; and not only the pores of that part to which the cold water was applied, but also of the whole body, are contracted. Do not these examples shew that the *dartos* and skin are affected

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ed by *stimuli*, and consequently irritable, though not in the same sense that the muscles are? The irritability of the parts of the human body, therefore, may perhaps be not improperly distinguished into three kinds: *viz.* That power of alternate contraction, which is peculiar to those organs, we call muscles; that uniform constriction which happens to the *dartos* and pores of the skin; and that redness and inflammation which is excited in every part of the body that is sensible, as often as acrid things are applied to it; although indeed this last is only an effect of the first kind of irritability in the small vessels of the parts.

As to the second argument, *viz.* That irritability is not observed to be in proportion to sensibility, our author has been very unlucky; since an inflammation of any irritable organ, which increases its sensibility, is always observed to make it more irritable, as will be shewn afterwards by a variety of examples. The Doctor, however, in proof of his assertion, tells us, that the stomach is more

more sensible than the intestines, and yet less irritable; and that the heart itself is endowed with no acute feeling, and, when touched in a living person, occasions fainting rather than pain\*.

THE stomach has a particular feeling whereby it is very disagreeably affected by things, that, as far as we can judge by our taste or smell, have very little acrimony: it is the principal seat of hunger; and, as when we have wanted food for any considerable time, it is affected with a more disagreeable sensation than the guts, so likewise it is more sensible of an agreeable feeling from grateful food: and in these respects, it may be said to be more sensible than the intestines. But, notwithstanding this, the intestines seem to be as susceptible of pain as the stomach, or indeed, any other organ of the body; an inflammation in them is as painful, if not more so, than in the stomach; and jalap, *senna*, and other smart purgatives, which seldom occasion any

\* Aët. Gœtting. vol. 2. p. 136.



any pain in the stomach, often affect the guts with severe gripings.

WITH regard to the heart; Dr *Harvey* seems too hastily to have concluded it to be void of feeling, because the young Nobleman, whose heart he touched, scarce felt any thing at all; for, what this great man put his fingers to, was not the substance of the heart itself, but an insensible *callus*, covering and defending it\*. The truth of the matter is, that, as the skin, although one of the most sensible parts of the body, feels no pain from a slight pressure or attrition,

\* Because a testicle covered with a *callus* or fungous substance is sometimes very sensible when touched, *M. de Haller* seems to think that the small degree of feeling in the heart of that nobleman mentioned by *Harvey* could not be owing to its callous covering\*. When a part which is covered with a *callus* is inflamed, it will doubtless be painful when touched; nay pressing even the nail gives pain when the nerves below it are inflamed: But to say in general that any part should be rendered more sensible by its being surrounded with a *callus*, is such a paradox as it would be to affirm that the fingers of a blacksmith feel more acutely than those of a delicate woman.

\* *Element. Physiolog.* vol. 1. p. 489.

tion, because it is defended by the insensible *epidermis*; so the heart, when gently touched, feels little, because it is covered with the inner *lamina* of the *pericardium*, which, like other membranes of the body, has but a small degree of sensibility \*. In like manner, the external surface of the intestines is rendered less sensible than it would otherwise be, by their being involved in the mesentery; and hence it is, that the woman mentioned by *Peyerus* felt no pain when her intestines were handled by him and *Wepferus* †. But, although the outer surface of the heart and intestines may have no great degree of sensibility, it will not thence follow, that their internal surface, where the natural *stimuli* exciting their motions act upon them, is not endowed with a more exquisite feeling: nay the contrary is highly probable, if not altogether certain. *M. de Haller* himself has observed, that the heart is much more affected in animals dying, or newly dead, by the gentle *stimulus* of warm water  
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AB. Gotting, vol. 2 p. 130.

† *Parerg. anatom. exercitat. 1. cap. iv.*

or air pushed into its ventricles, than by applying the most acrid liquors to its external surface, or even pricking it with the point of a knife \*; and it will appear from an experiment to be mentioned afterwards, that, in some cases, the *stimulus* of the blood within the cavities of the heart will excite a tremulous motion in this organ; when oil of vitriol applied to its external surface has not the least effect this way.

WITH regard to the comparative sensibility and irritability of the heart and intestines, it is not easy to say any thing certain, nor is this needful; since from our author's experiments it does not appear clearly, whether the heart or intestines are most irritable†. The motions of the heart are indeed stronger and more frequently repeated; but those of the intestines continue, in many animals, as long, if not longer, after death.

As for our author's third argument, *viz.* that parts destitute of feeling are irritable;

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there

\* Aët. Gotting. vol. 1.

† Aët. Gotting. vol. 2. p. 147.

there is not so much as one instance given, nor indeed can be given, of a part being irritable that is naturally insensible and destitute of nerves\*: but what he thinks equivalent to this, is, that muscles continue to be irritable, not only for some time after their nerves have been tied or cut, and so all communication between them and the brain intercepted, but also after they have been intirely separated from the body. And, indeed, it must be owned, there is a great deal of seeming weight in this argument: but that it is, nevertheless, inconclusive, has been already shewn in the last section of my *Essay on the vital and other involuntary motions*

of  
\* Our author indeed mentions, upon the authority of *Lupsius*, the secundines and membranes of the *ovum* as irritable, and yet destitute of nerves. But, if irritability, as he himself allows, be a property of muscular fibres alone, it will follow, that the membranes of the *ovum*, which are not muscular, cannot be irritable: but, supposing they were both the one and the other, it is not a clear point, that they may not be supplied with small nervous filaments propagated to them by means of the navel-string.



of animals ; and will, I hope, appear still more so from the following considerations.

I. ALTHOUGH the irritability of muscles continues, in a small degree, for some time after their nerves are tied or otherwise destroyed ; it will not follow, as our author thinks, that this power does not depend upon, or proceed from the nerves : for, if this were so, one would expect that, in a living animal, where the muscles are all supplied with blood by the arteries, they should continue to preserve their power of irritability, not only for a few minutes, but for many hours and days after their nerves have been tied or cut. Further, if the irritability of the muscles were not owing, some how, to the nerves or their influence, why should an irritation of the nerves or *medulla oblongata*, produce such remarkable convulsions ?

THESE convulsions cannot be owing to the propulsion of any subtile fluid in the nerves, towards the muscles ; since, as *M. de Haller* and others have observed\*,

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these

\* Aët. Gotting. vol. II. p. 136.

these motions follow equally whether a nerve going to any muscle is squeezed upwards or downwards. If they were owing to the connexion or vicinity of the nerves to the muscles, one would expect, that more remarkable convulsions should follow from an irritation of the tendons, than of the nerves: the contrary of which, however, is true; for, while the irritation of a nerve produces stronger convulsive motions in the muscles, than arise even from the laceration of their own fibres, the tendon, however pricked or irritated, produces no change in them †. The reason is plain; the tendon has little or no feeling, while the nerve has a very acute one.

BUT further, it ought to be observed, that when, after decollation, a frog's spinal marrow is destroyed with a red hot wire, no visible motion is produced in its limbs, or body, by pricking, cutting, or otherwise hurting them: only, when the skin of the thighs was dissected off, and the muscles were irritated, their fibres were agitated

† Aet. Gotting. vol. ii. p. 140.

gitated with a weak alternate tremulous motion. Now, as the strong convulsive motions, excited by irritation in the legs and trunk of the body of a frog after decollation, are certainly to be ascribed to the intire state of the spinal marrow, since they cease as soon as it is destroyed; Is it not highly probable, that the weak tremulous motion in the irritated muscles of a frog's thighs, after the destruction of the spinal marrow, were owing to the influence or power of their nerves, which still remained intire\*? It seems also to deserve particular notice, that, after the destruction of the spinal marrow, altho' the fibres of such muscles as were irritated, exhibited a weak tremulous motion; yet there was no sympathy between the different muscles,

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or

\* As the alternate motions of the heart, in many animals, continue for a long time after the destruction of the brain and spinal marrow; Is it not probable, that its nerves are so constituted as to make its moving power less dependent on immediate supplies from the brain and spinal marrow, than that of the voluntary muscles?

or other parts of the body, as was observed, while the spinal marrow was intire : from whence it seems to follow, that the nerves distributed to the several parts of the body, have no communication but at their termination in the brain or spinal marrow ; and that to this, perhaps alone, is owing the consent or sympathy observed between them.

UPON the whole ; the weak alternate motions, produced by irritating muscles whose nerves have been tied or cut, by no means prove, that their irritable power is independent of the nervous influence : they only shew, That these motions are not owing to any new derivation of spirits from the brain into the muscles at that time—that the presence of the nervous influence in their fibres is only requisite , and that the spirits remaining in the nerves, below the ligature and in the muscular fibres, may be sufficient to preserve a certain degree of irritability, or power of motion in them, for some little time.



*M. de Haller* further concludes irritability to be independent of the brain and nerves; because the smallest insects, which have no head, are irritable\*: but, if this argument was good for any thing, it would prove sensibility and voluntary motion to be also independent of the brain and nerves; for the smallest insects seem to be endowed with feeling, and undoubtedly perform voluntary motions. May not these insects which want a head, have something to supply the place of a brain, from which the nerves may take their rise? Or may not the nerves be so formed in them, as to be sufficient of themselves, without a brain, for the purposes of motion and sensation? Arguments of this kind, which are drawn from our ignorance of the true structure of animals, can be of no weight.

2. *M. de Haller*, while he denies feeling to the *dura* and *pia mater*, allows it to the medullary substance of the brain†; because

\* Act. Gotting. vol. II. p. 156.

† Act. Gotting. vol. II. p. 130. et 134. et primæ lineæ Physiolog. 2d edit. p. 238.

cause, when it is wounded, the muscles of the body are convulsed in an extraordinary manner. Now, if the sensibility of the medullary part of the brain in living animals may be deduced from the convulsive motions which ensue upon hurting it: Are we not (our author himself being judge) to ascribe feeling to the brain, even in animals newly killed; since in these, the motion of the heart is renewed by irritating the *medulla oblongata*, and the whole muscles of the body are convulsed by dissecting the spinal marrow †? And altho', in animals newly dead, the convulsive motions produced by irritating the *medulla oblongata* or *spinalis* be weaker and less remarkable than in living animals, yet it will by no means follow, that they are not indications of sensibility, and owing to the same cause as in living animals: for, as the death of the body, in general, soon puts an entire end to every kind of feeling and activity in the parts of most animals, so it is not to be doubted, that, immediately  
after

† *Kauu impet. faciens*, No. 330. et 333.

after death, these powers begin to be weakened; wherefore the motions owing to them must be less remarkable.

FURTHER, if the convulsions occasioned by irritating a nerve in its natural state are allowed by all to be a proof of its feeling, the like, tho' weaker, convulsions excited in the muscles by irritating a cut or tied nerve must be an equal proof of its still retaining, in some measure, its sensibility. When all communication, therefore, with the brain, by means of the nerves, is cut off, convulsive motions, which arise from a *stimulus* applied to any part, are equally a proof of the sensibility of that part, as if the communication were preserved. And, if in the latter case, these motions may be justly ascribed to the nerves, being hurt by the irritation, they must be equally so in the former.

3. But here it will be objected; How can there be any sensibility or feeling in a nerve, whose communication with the brain is cut off?

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IN answer to which, it may be sufficient to say, That, since we have strong reasons for believing that the parts of many insects continue to be sensible for a considerable time after they have been divided from each other \*; and that the bodies of some larger animals continue to live and feel after they are deprived of their heads †: Why may not the muscles of men preserve some degree of sensibility for a few moments after their nerves are tied or cut, although we may not be able to account for this, from any thing we know of the nature of the body, or of the manner in which the soul is present with, or acts upon it ‡?

*Redi*

\* Flies copulate and lay eggs after decollation. *Boyle's usefulness of experimental philosophy, part 2. pag. 16.*

† Vipers continue for three days after being deprived of their head and heart, to be manifestly sensible of punctures, and move their bodies, when pricked, just as entire vipers do. *Boyle's usefulness of experimental philosophy, part 2. p. 16.*

‡ If I were allowed to indulge myself in conjectures concerning a matter of which I know very little, I  
would



*Redi* tells us, that the head of a viper will bite half an hour after it is cut off from the body, (*vid. Jacobæi observat. de ranis et lacert. p. 58.*); and I have often observed, that

would say, that, although there can be no feeling or perception in the brain when a nerve is pricked below where it is cut or tied, yet, if the soul be present every where in the body, as seems highly probable, there may be some kind of feeling or sensation excited in the nerve itself, which may be sufficient to produce a motion in the muscles to which it belongs.

Dr *Stewart* has produced several arguments to prove, that the inferior extremity of every nerve is to be considered as the brain of the organ or part in which it terminates; and that the soul is not confined to the brain or any part of it, but is present every where in the body, equally in the extremities of the nerves, as at their origin. (*Dissert. de motu muscular. cap. v.*) And if this be so, as it may for any thing that can be shewn to the contrary; why may not a muscle, whose nerve is tied or cut, continue, for some little time, sensible and irritable? Its sensibility will not indeed be attended with what is properly called *consciousness*, as distinguished from *simple sensation*: because this reflex act, by which a person knows his thoughts or sensations to be his own, is a faculty of the soul only exercised in the

that a frog's head, after being separated from its body, not only continued, for above half an hour, to move its eye-lids, nostrils, and muscles of the lower jaw, when its brain or  
the

the brain, with which all communication is now cut off.

As the soul seems to imagine, judge, reason, and remember in the brain only; so, why may it not have, in the various other parts of the body, such feelings or powers as are necessary for carrying on their several functions? Particularly, why may it not, in the muscular fibres, have the power of simple sensation and of beginning motion? Or, which will amount to much the same thing, while the rational soul acts only in the brain, there may perhaps be, as some have thought, a sentient active principle, which enlivens the whole body, and which continues to actuate the parts, for some time after their communication with the brain is stopt, *i. e.* as long as they continue in due order for being acted upon by it.

The more probable opinion, however, seems to be, that the soul is equally present in the extremities of the nerves through the whole body as in the brain. In those, it is only capable of feeling or simple sensation; but in this, it exercises the powers of reflex consciousness and reason. When the communication of any part with the brain is cut off, the simple sensation of feeling  
excited

the skin of its head was touched with a probe, but sometimes moved its eyes and eyelids, when nothing touched it, and as it were of its own accord; so that, without an ob-

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stinate

excited in such part is no longer perceived by the soul in the brain; and therefore is not attended with reflex consciousness: the nerves being now also separated from the brain, soon become unfit to perform their functions; hence the powers of simple sensation and motion in the part, if it be muscular, cease by degrees, till at last it becomes quite dead. The communication, therefore, between the several organs and the brain, is not only necessary to preserve their nerves, by means of some influence transmitted to them, in due order for performing their functions and being properly affected by their several objects, but also, that the soul, as a conscious and rational being, may be acquainted with these impressions.

It will be unfair to object here, that we ascribe the intelligent powers of the mind to the bodily organs; for as the best musician cannot make a flute give the sound of a violin, nor a harpsicord that of a French horn, nor without these several instruments produce their sounds and notes at all; in like manner, the soul, in the *present* state, can only exercise its rational powers in the brain; it can only taste in the tongue, smell in the nose, see in the eyes, hear in the ears, and feel hum-

... .. ger

stinate degree of scepticism in this matter, we cannot deny that the head continues to be animated for a considerable time after it is separated from the body, and to perform not only involuntary motions when stimulated, but, in appearance, also voluntary ones. In like manner, the body of a frog, after being divided from the head, preserves the power of motion, for above an hour; and when its hind feet or toes are cut, or otherwise hurt, the muscles of its thighs, legs, and trunk are strongly contracted, by which it

ger in the stomach. But although the imagination, memory, and rational powers, depend upon the brain: yet the brain does not imagine, remember, or reason: although taste depends on the tongue, smelling on the nose, seeing on the eyes, and hearing on the ears; yet these organs neither taste, smell, see, nor hear, but only that living sentient principle which animates them.

It may be proper to observe, that, whether these conjectures, which are offered with a great deal of diffidence, shall be thought probable or not, the argument concerning the irritable power of the muscles of animals will not be materially affected; since this must be determined, not by metaphysical reasonings, but by experiments and observations. Vid. sect. iv. below.



it raises its body from the table, and sometimes moves from one place to another. When the muscles of the thighs are pricked or cut with a knife, they are excited into contraction; but neither they, nor the neighbouring muscles, are near so strongly convulsed as when the toes are wounded: Whence should this happen; and why should not the muscles of the legs and thighs be more strongly convulsed, when they themselves are wounded, than when the toes are treated in the same manner? This would undoubtedly be the case, if the motions of irritated muscles were owing to some property of the insensible matter composing them. But if, as we imagine, they are all to be derived from feeling, it is easy to see, that, as the feet and toes are more sensible of pain when wounded, than the muscles of the legs or thighs, stronger convulsions must be occasioned by an irritation of the former, than of the latter.

FURTHER, we must either allow that both the head and body of a frog continue to be animated, for a considerable time, af-

ter they are separated from each other ; or else affirm, that the life, feeling and active powers of animals, are merely properties of that kind of matter of which they are composed. The former opinion, is attended with some difficulties, which arise *solely* from our ignorance of the nature of immaterial beings : the latter, is inconsistent with all that we know of matter or its properties. If we admit it, therefore, we not only ascribe qualities to matter which it does not possess, but presume to limit, by our scanty and inadequate capacities, the powers of incorporeal natures, their manner of acting upon bodies, and co-existing with them.

If the soul were confined to the brain as many have thought\*, Whence is it that a pigeon not only lives for several hours after being deprived of its brain, but also flies from one place to another † ? And to what cause are we to ascribe the continuance of life and motion, in a viper for three days after its head is cut off, and in a tortoise for three weeks

\* Act. Gotting. vol. 2. p. 153.

† Baglivi opera 4to. prefat. p. xi.

weeks after decollation, and six months after the loss of its brain †? The motions performed by these animals cannot surely be attributed to their material part alone; unless we shall deny them a soul altogether, and, with *Des Cartes*, refer all their actions to their corporeal machinery. The late Reverend and Ingenious *Dr Hales* informed me, that, having, many years since, tied a ligature about a frog's neck, to prevent any effusion of blood, he cut off its head, and, thirty hours after, observed the blood circulating freely in the web of the foot: the frog also at this time moved its body when stimulated: but, on thrusting a needle down through the spinal marrow, the animal was strongly convulsed, and, immediately after, became motionless.

If then the soul in pigeons, frogs, vipers and tortoises, is by no means confined to the brain, but can continue for a long time to actuate their bodies independent of it; and if, in many insects which have no brain, every part of the body is both sensible and

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irritable;\*

† *Redi observat. circa animal vivent. p. 209. &c.*

irritable \* ; why should we deny, that, in man and such animals as resemble him most, the parts may continue to be actuated by the soul or sentient principle for some few minutes after their communication with the brain has been cut off \* ?

IF any man of ordinary sense, who is no philosopher, be asked, Why the heart of a frog beats after being separated from the body, and renews its motions when pricked ; he will readily say, It is because there is *life* in it ; and this is a proper answer ; nor can a better, perhaps, be given by the ablest philosopher. If then *life* in animals be owing to the energy of a principle distinct from matter, and of powers superior to it, we have reason

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\* Aët. Gotting. vol. 2. p. 138.

† The difference between men and those animals which live long after decollation or the excision of their heart, seems to be, that the latter are so framed that fresh supplies of blood and spirits from the heart and brain are not immediately necessary to keep the several parts in due order to be acted upon by the soul ; as seems to be, in a great measure, the case in man and many other animals.



to conclude, that, as long as any signs of life remain in the bodies of animals, or any of their parts, this principle still continues to actuate them.

THERE are two kinds of motion from irritation, observable in living animals: viz. where the muscle or organ itself is stimulated, and where the *stimulus* only affects some neighbouring or distant part. The first (of which kind is the motion of the heart) seems to be owing to the soul or sentient principle as acting in the part moved; but the second, to the soul as perceiving and acting in the brain: and of this kind is, the motion of sneezing from an irritation of the nose, and the contraction of the diaphragm in vomiting and in a *tenesmus* or stranguary. In order to the first kind of motions, an immediate communication with the brain is not absolutely necessary, but only such a share of the nervous power in the muscle or its nerves, as may be requisite to fit its fibres for being acted upon by the soul or sentient principle. But the case is quite otherwise in the second;

second; where the motion produced is through the intervention of the brain, and not by any *stimulus* applied to the part moved. And hence it is, that, in an animal newly dead, the diaphragm is not brought into contraction by lacerating or pricking the *intestinum rectum* or neck of the bladder, altho' the fibres of these parts themselves may be, thence, agitated with some tremulous motions. In like manner, though the muscular coat of the stomach is excited into contraction some time after the death of the animal, by irritating it; yet the diaphragm is no ways affected by this irritation: which, however, it would have been, if the animal had been alive. Agreeably to this, when any of the muscles of a frog's legs are irritated some time after cutting off its head, almost all the muscles belonging to the legs and thighs are brought into contraction, if the spinal marrow be entire: but, as soon as this marrow is destroyed, although the fibres of such muscles as are themselves stimulated are affected with

a weak tremulous motion; yet the neighbouring muscles remain altogether at rest.

I have elsewhere endeavoured to shew, That the supposition of the soul or sentient principle's continuing for some time to actuate the separated parts of animals, does not infer its *real* divisibility\* ; nor is it necessary to repeat the same things again : but, I cannot help observing, that, when *M. de Haller* represents me as holding the soul to be divisible, so as that it may be cut into as many pieces as the anatomist pleases† : he charges me with an opinion which I not only do not maintain, but which I have brought arguments to disprove. I shall only add, that the indivisibility of the soul does not depend on the unity of the body, but on its own particular nature.

It must be acknowledged, that there is a great deal of obscurity in these matters : but, as, in every part of nature, we find abundance of mysteries, as often as we push our inquiries to any great depth ; it can be no wonder if we meet

\* Essay on vital motions, &c. p. 380. &c.

† Act. Gotting. vol. 2. p. 137.

meet with difficulties, almost insurmountable, in accounting for the motions of animals, or tracing them up to their first source : for, if we are far from understanding the communication of motion and other actions of *matter* upon *matter*, How shall we able to comprehend the manner in which an *immaterial* principle acts upon it ? But, as we can, from the little we know of matter, see that inactivity is one of its essential properties, we are hence convinced of the necessity of ascribing the life and motions of animals to the power of an *incorporeal* Agent.

If we knew the manner of existence of the soul, or the way in which it acts upon, or is present with the body ; it would be a very proper objection to any physiological opinion, that it was inconsistent with what we certainly knew of these things : but, as we are utterly ignorant of them, it is highly unreasonable and absurd to argue against an opinion supported by experiment and analogy, from its supposed inconsistency ; with what ? why, truly, with nothing ! For what we are totally ignorant of, is, to us,



as if it were nothing; and we can neither affirm nor deny any thing to be either consistent or inconsistent with it.

### SECT. III.

*M. de Haller*, after endeavouring to prove that irritability is independent on sensibility, gives it, as his opinion, That this remarkable property of the muscles, has its seat in the glutinous matter connecting the earthy elements of which their fibres are composed\*; and that irritability ought to be looked upon as a particular property of this glutinous substance, in like manner as gravity is allowed to be a property of matter in general, altho' its cause cannot be assigned†.

BUT surely the glutinous matter of the muscles of animals seems as unlikely to be endowed with an active power, such as irritability, as any other constituent part of the animal body; nor can any thing be deduced

\* *Aët. Gotting.* vol. 2. p. 152.

† *Ibid.* p. 154. and 157.

ced from its endeavouring to shrink or shorten itself when drawn out \*; for the glue of the skin, ligaments and tendons, as well as of the muscles, has this property, which is, indeed, a kind of elasticity †, and no way similar to that power of alternate contraction which muscular fibres are endowed with.

THE Doctor, in proof of his notion of the irritable nature of the muscular glue, adds, that young animals which abound most in it are most irritable. The observation is certainly true, but proves nothing in the present case; for the skin, ligaments, and tendons (which last are a continuation of the muscles, only harder and more compacted) abound much more in glue than the muscles, and yet are not in any degree irritable. The greater irritability of the fibres of young animals

\* Act. Gotting, vol. 2. p. 152.

† Elasticity is not a property of hard bodies alone, as *M. de Haller* seems to think (p. 152.), but is also found in soft ones: thus air, wool and the down of feathers are remarkably elastic.

animals is to be deduced from their greater sensibility, and this is owing to their greater softness and tenderness : thus, what in new-born animals is a sensible and irritable muscle, becomes afterwards a tendon, which, in a sound state, is destitute of irritability, and endowed with little or no feeling \*.

BUT further, since the gelatinous matter in our aliments, and even in our blood, is quite destitute of the property of irritability, it must owe this power to the particular disposition or arrangement of its parts, or to some other change which it suffers, when it becomes a part of a muscle : And if this may be so, why may not the finer and more subtile parts of the blood be so changed in the brain, as there to acquire a power of feeling and thinking, *i. e.* if *irritability* be a property of the muscular *glue*, why may not sensibility and intelligence be properties of the medullary substance of the brain? for the known properties of matter give us reason to think, that

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real

\* Gottingenf. vol. 2. p. 140.

real activity is not more consistent with its nature, than feeling or thought.

BUT it has been said, that irritability may be a property of the muscular glue, as well as gravity is a property of matter in general : let us therefore consider this notion a little, and see whither it will lead us. Gravity, which is a property of matter, continues to be so, let matter be ever so much altered or changed by fire, menstruums, or other causes ; but, when the *gluten* of the muscles is extracted from them, it appears as inert and destitute of active powers as any other matter ; nay, tho' allowed to remain in them, yet, in most animals, it loses its power altogether very soon after the muscles are separated from the body.

BUT, supposing irritability to be a property of the muscular glue in the same sense that gravity is a property of all matter, yet, as the most attentive consideration of the nature of matter has convinced philosophers that gravity is not essential to it, but owing to some general cause acting upon it ; so the irritability of the muscular glue



glue must be allowed not to be a property essential to it, but arising from the action of some other cause upon it. Gravity has been ascribed either to the immediate and continued operation of an *immaterial* being, or to the action of some subtile elastic *medium* on matter: But, since the elasticity of the parts composing such a *medium* must be, at last, referred to the active power of some *incorporeal* cause, it follows, that gravity must be so likewise\*.

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\* *M. de Haller* has represented me as maintaining an error contrary to the *common notions* of mankind (a), when I say, that gravity, or rather the elasticity of that *medium* supposed to be the cause of gravity, must be, at last, referred to the active power of some *incorporeal* cause. How far we should be safely directed in our reasonings concerning the nature or cause of gravity by the common notions of mankind, I shall leave the reader to determine; but some very able philosophers have thought, and not without reason, that gravity, or the material cause producing it, must at last depend upon the power of that BEING, who sustains, moves, and governs the whole system of

(a) *Memoires sur les parties sensibles*, &c. vol. iv. p. 91.

It appears, therefore, after all that has been said to shew that the motions of irritated muscles are owing to a property of irritability in them or their glue, that we are at last obliged to refer them to the active power of an *immaterial* cause; unless we shall, contrary to all sound philosophy, choose to ascribe feeling and proper activity to matter. And, as *gravity* must finally be resolved into the power of that BEING who upholds universal nature; so it is highly probable, that the irritability of the muscles of animals is owing to that living sentient principle, which animates and enlivens their whole frame.

## SECT.

of nature: and *M. de Haller* will find it a hard task to account for the elasticity of that æthereal *medium* supposed to be the cause of gravity, from any thing we know of the nature of matter, or of the properties that are essential to it.

S E C T. IV.

HAVING thus endeavoured to lay open the insufficiency of *M. de Haller's* theory of irritability, I shall conclude with a few observations, which, if they do not demonstrate, make it, at least, extremely probable, that the motions of stimulated muscles proceed from their sensibility, or are closely connected with it: but, previous to these, I must be allowed to take notice, that the word *irritability* seems to imply a kind of life or feeling in the part endowed with it, which renders it capable of being fretted, provoked or irritated, and therefore seems to be improperly applied to express the contractile power of stimulated muscles, if this power has no connection with, or dependence on their sensibility\*.

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\* *Quicquid incommodum, alicunde illatum percipit, idemque a se amoliri satagit, id proprie dixeris irritari. Idcirco cum partes istæ injurias persentiscant, seseque ab iisdem vindicare conentur, irritationis quoque capaces merito dicendæ sunt. Glisson. anatomi. hepat.*

We never talk of irritating a stone, a piece of wood, a tree, or indeed any thing that is destitute of feeling. *Irritability*, therefore, in the common acceptation of the word among mankind, implies some kind of feeling; nay *M. de Haller* himself, notwithstanding his professed design is to shew irritability to be independent of sensibility, speaks once and again of parts that are not irritable, as not feeling or perceiving the acrid matter, or other *stimulus* applied to them \*. So true is the observation of the poet,

*Naturam expelles furca; tamen usque  
recurrat.*

BUT to return ;

I. WE almost always observe, the irritability of the muscles or muscular organs of the human body to bear a proportion to their sensibility. Thus, in young children, where the tender nerves and fibres are more easily hurt, and all the feelings are more exquisite; the quickness of the pulse and

\* *Acta Gottingenf.* vol. 2. p. 142.



and the violent convulsions with which they are often affected, from very slight causes, shew their muscles to be endowed with a greater degree of irritability, than those of adults \*. In like manner, grown people of delicate nerves and very quick feelings are subject to spasms and convulsive motions of their stomach, intestines, &c. and to palpitations of their heart, from such slight causes as would scarce sensibly affect men of firmer constitutions and less moveable nerves.

ON the other hand, in old people in whom all the feelings become less acute, the muscles are less irritable; witness the slow motion of their heart. And, in apoplectic and comatous cases, where the senses are greatly impaired, the heart's motion, and that of respiration, are remarkably slow; and the *stimulus* of the *feces* is not sufficient to

\* It may also be observed, that the parts of young animals which are most sensible, are not only most irritable, but retain their power of motion longest after death, or separation from the body. Vid. Essay on vital motions, p. 358.

to bring the intestines, diaphragm, and abdominal muscles into contraction, as usual.

FURTHER, the nerves, which are the most sensible parts of the body, produce, when irritated, the most remarkable convulsive motions in the muscles; and, when they are, by being stretched, rendered more susceptible of pain, an irritation of them produces still greater convulsions\*.

II. WHATEVER increases the sensibility of the muscles or moving organs of our body, also increases their irritability.

THUS, when the stomach is inflamed, the mildest liquors, received into it, are apt to provoke vomiting, or the hiccup; whereas, in a sound state of this organ, brandy, vinegar, and other acrid liquors, produce no such effect. When the neck of the bladder is slightly inflamed or excoriated, the urine, which used to give little disturbance till collected in large quantity, irritates this tender part, so as to produce violent and often-repeated efforts to empty the bladder.

WHEN

\* A. G. Gottingen. vol. 2. p. 136.

WHEN the *fauces* are attacked with an inflammation, the muscles of deglutition are more strongly convulsed in swallowing, than when these parts are in their natural state. When the intestines are by any means deprived, in a good measure, of their *mucus*, or rendered more sensible by a very slight degree of inflammation in their inner membrane; the gentlest purgatives often operate as severely as the stronger ones do in a person in perfect health. When, without any degree of erection in the *penis*, the *semen* escapes into the *urethra*, the *musculi accelatores urinae* are no ways affected by it: but, as often as the *penis* is erected, and thereby its parts rendered more sensible, and, as it were, half inflamed, the *semen* is no sooner poured into the beginning of the *urethra*, than the above mentioned muscles are excited into strong convulsive contractions.

THE heart becomes so irritable, when itself or the *pericardium* is inflamed, as to be agitated with violent convulsions and palpitations. Nay, the tendons, which, in a sound state, have little or no feeling, and

are

are not irritable<sup>\*</sup>; become, when inflamed, so sensible of *stimuli*, that the most violent convulsions have been occasioned by pricking, tearing, or otherwise irritating them.

A disagreeable sensation in the stomach from wind and other causes, often quickens (especially in people whose nervous system is very delicate and moveable) the motion of the heart; which will be often rendered slower again, by a glass of generous wine, a dram of brandy, or any thing that, by invigorating the stomach, removes the uneasy sensation in it.

A disagreeable feeling in the stomach renders the heart more irritable, because, by means of its nervous sympathy with this organ, it increases its sensibility; and, in like manner, an inflammation or unusual irritation in the kidneys or intestines increase the irritability of the stomach: but how a disagreeable feeling in the stomach should immediately alter the nature of the *gluten* of the fibres of the heart, in which *M. de Haller* places the irritability of this organ, is as

\* Aët. Gotting. vol. 2. p. 140.



inconceiveable, as it is inexplicable upon any just principles of physics.

If therefore it appears, that the irritability of the moving organs of our body is increased as often as their own sensibility, or that of other parts with which they have a remarkable sympathy, is increased; it will be thought, at least, highly probable, that the irritability of any part depends upon its sensibility.

III. WHATEVER lessens or destroys the sensibility of the muscles of animals, also lessens or destroys their irritability or power of motion.

Thus, when one's fingers or limbs have been long exposed to severe cold, they not only become insensible, but paralytic. Frogs, bats, and other animals, with numbers of the insect-tribe, are so benumbed by the winter's cold, as to be deprived of all feeling and motion: their blood does not circulate, nor their hearts beat; and their muscles, tho' torn, cut in pieces, or otherwise stimulated, are not brought into contraction.

DURING

DURING the time of incubation, the chick's heart is observed to beat faster or slower, and with more or less force, *i. e.* to become more or less irritable, as it is exposed to greater or less degrees of heat; nay, after its motion has been stopt altogether by cold, a gentle heat will make it, in a very short time, begin to contract a new \*.

FURTHER, this *punctum saliens*, or heart of the chick, which, when touched with any thing capable of hurting it, is excited into quicker and stronger contractions, after being exposed for some time to too great cold, is not affected by the most powerful *stimuli*.

It appears, therefore, that feeling and irritability are destroyed by cold, and restored by a proper degree of heat, and are so closely connected together, that the latter is never to be found where the former is totally wanting.

IF authority could be of any weight in a matter which is to be determined by experiments

\* Harvey de generat. animal. exercit. xvii.

riments and observations, we might support our opinion with the name of one of the most judicious and successful inquirers into nature, that any age has produced. "Ego pluribus experimentis certus sum" (*says the illustrious Harvey*), non motum solummodo puncto salienti inesse, sed sensum etiam; nam, ad quemlibet, vel minimum, tactum, videbis punctum hoc varie commoveri, et quasi irritari.—Vidi, inquam, sæpissime, aliique qui una mecum aderant, ab acùs, styli, aut digiti contactu, immo vero a calore aut frigore vehementiore admoto, aut cujuslibet rei molestantis occurso, punctum hoc variis sensûs indicia, pulsum nempe varias per mutationes, ictusque validiores ac frequentiores, edidisse; ut non dubitandum sit, quin punctum hoc (animalis instar) vivat, moveatur, ac sentiat." De generat. animal. exerc. xvii.

UPON occasion of quoting Dr *Harvey*, it may not be improper to take notice of the error of those who seem to think the irritability

bility of the muscles a late discovery\*. If by *irritability* is meant that power by which muscles contract when they are pricked, fretted, or otherwise stimulated, 'tis plain this was not unknown to *Harvey*; and many authors since his time might be named, who have particularly mentioned it †. But,

\* *Tissot. discours preliminaire sur l'irritabilité*, prefixed to his translation of *M. de Haller's* treatise of the sensible and irritable parts.

† The irritability of the heart, after being separated from the body, was not unknown to *Galen* (a), but it has been more particularly attended to by several physicians and philosophers since the middle of the last century.

*Swammerdam* tells us, that, in dissecting animals alive, he observed not only contractions in the muscles, but in every muscular fibre, though separated from the body of the muscle. *Tract. de respiratione*, cap. vii. § v. 1667.

*Dr Glisson's* treatise *de anatomia hepatis*. (1654) evidently shews, that he was well acquainted with the irritability of the muscles; and, in his book *de ventriculo et intestinis*, (1677), he has several chapters on the irritability of the parts of the body; where he

(a) *Cor quamvis ex animali extractum fuerit, motionem diutius, tum in contractione, tum in dilatatione servare conspici. De Hippocrat. et Platon. decret.*



if by *irritability* be meant an active property of the muscular glue analogous to gravity; this, it must be confessed, is a new discovery, though not likely to prove a lasting one.

R 2

*Opinionum*

not only mentions the heart and intestines as endowed with this property, but tells us particularly that the fibres of the muscles in dead animals are brought into contraction when acrid liquors are applied to them, cap. vii. No. 3. He gives several examples of the irritability of parts from sympathy, and mentions the causes which may produce either too small or too great a degree of irritability in the fibres, cap. ix. No. 4, 5, 6, & 7. He supposes irritability to arise from a natural perception in the fibres, without which they could be no more affected by any irritating cause than a deaf man is by sounds. This natural perception he distinguishes from feeling, concerning which he reasons at great length, but with little perspicuity, cap. vii.

*Peyerus*, after endeavouring to confute *Harvey's* opinion of the chick's heart being not only endowed with motion, but also feeling, and ascribing the irritability of this organ to its exquisite but unknown structure; adds, "Constat vero piscium plurimos, nec non insecta  
"et alia quædam animalcula motus sui aut vitæ ad-  
"modum esse tenacia, adeo ut in partes quoque dissec-  
"ta sese aliquamdiu adhuc motitent, imprimis si adhi-  
"bito

*Opinionum commenta delet dies, naturæ judicio confirmat.*

BUT, to return from this digression;  
*Opium*, which is remarkable for its power of  
 impairing

“bito stimulo, insuper laceffantur. Parerg. anat. med.  
 “7mum pag. 200. Genev. 1681.” The irritability  
 of the intestines and heart was so well known to *Boh-  
 nius*, that he deduces the peristaltic motion of the in-  
 testines from the irritation of the aliment, and ascribes  
 the alternate contraction of the heart, partly to the  
*stimulus* of the blood rushing into its cavities, which  
 had been mentioned before by *Harvey* and *Glisson* as  
 the sole cause of the heart’s motion. *Circul. anat.  
 physiolog.* p. 105, & 163. edit. 1686.

*Baglivus* has, in his book *de fibra matrice*, an intire-  
 chapter *de irritatione solidorum sive stimulis et variis  
 stimulorum effectibus*: from which it appears, that he  
 was far from being ignorant of the power of *stimuli* to  
 excite the parts of living animals into contraction. He  
 has also several experiments concerning the irritability  
 of the heart after being separated from the body, and  
 mentions particularly that frogs are convulsed by punc-  
 tures an hour after they have been deprived of all the  
*viscera* of the *thorax* and *abdomen*. *Exper. xi. de cir-  
 culatione sanguinis in rana.*

Among the later writers, Dr *De Gorter* has, in ma-  
 ny places of his works, taken notice of the motions of  
 such

impairing or destroying the sensibility of all the parts of the body, also lessens or suspends the irritability or moving power of the muscles. Thus, in a small dose, it puts a stop

R 3

to

such parts of animals as are irritated ; and observes, that these motions are not to be accounted for from elasticity. “ Sed præterea cum omnes fibræ nervosæ vellicatæ sese inordinate et involuntarie moveant, patet minimam causam sæpe sufficere ad totam corporis œconomiam turbendam.—Cur autem a vellicatione pars aliqua nervosa statim contrahitur, difficile explicatur ; veritas autem ejus asserti ubique manifesta est, non modo in nervo isto vellicato, sed et in reliquis surculis nerveis ab eadem origine venientibus, ut in sternutatione, tussi, vomitu, &c. Sentio id esse adscribendum summi Opificis placito, qui voluit corpus nostrum ita concinnare, ut statim ac vellicetur pars nervosa, ibidem demandentur spiritus ; hoc enim ab elasticitate partium derivare, vellicatione vel stimulo agitatarum et oscillantium frustra tentarunt multi.” *Gorteri compend. medicina*, p. 58, & 63. Lugd. Batav. 1735.

Dr *Monro*, in his anatomy of the nerves, tells us, “ That all muscles, but especially the heart, continue to contract, in an irregular way, for some time after they are cut away from the animal to whom they belonged ; and that, after this motion of theirs has ceased,

to vomiting and coughing; and quiets the the convulsive motions of the *intestinum rectum*, bladder, abdominal muscles and diaphragm in a *tensismus* and stranguy, although the

“ceased, it may be restored again by breathing upon  
“them, or pricking them with a sharp instrument.”  
Anatomy of the human bones and nerves, p. 38. edit.  
3d, 1741.

*M. de Haller*, speaking, a dozen of years ago, of the motion of the heart in time of sleep, says; “Cæterum  
“tota theoria ista simplicissimo phænomeno, a nemine  
“negabili, nititur, omnem fibræ musculosæ ani-  
“malis vivi, irritatam a quacunque causa, continuo in  
“contractionem ire, ita ut hæc ipsa ultima nota sit,  
“qua animalia imperfecta a vegetabilibus dignoscantur.”  
And afterwards, with regard to the motion of the heart after its separation from the body, he expresses himself thus; “Omnino videtur quod alibi falsus sum,  
“cum PRÆCEPTORE, in fibræ animali aliquam ad irritationes contractilitatem superesse, quæ simplici elatere fortior, a motu muscolari diversa, quod cerebri  
“cordisque non indiga sit, et in ipsa hujus fibræ humi-  
“dæ adhuc et integræ fabricæ fundata esse videtur.”  
*Boerhaav. prælect. academ. vol. iv. p. 586, & 616, 1743.*

*Dr Winter*, in 1746, published an oration *de certitudine in medicina practica*, wherein, it is said, he has referred



the *stimuli*, which produced these motions, continue to act on the parts: when given in much larger quantity, it suspends the peristaltic motion of the intestines, and makes the heart

ferred all the motions of the human body to the irritable nature of the fibres and the power of a *stimulus*; acknowledging, however, with *Baglivi*, the *dura mater* as the fountain from which all our motions spring. But this piece I have not yet had the good fortune to see.

In an essay on the vital and other involuntary motions of animals published in 1751, the author, after considering particularly three kinds of contraction observable in the muscles of animals, *viz.* natural, voluntary, and involuntary from a *stimulus*, endeavours to shew, that all the vital and involuntary motions are owing to *stimuli*. irritating either the organs moved, or some part with which they have a particular sympathy; that the alternate contractions, excited in muscles by irritating substances applied to them, proceed from their sensibility, and are no more than an effort of nature to throw off what is hurtful: from which he concludes, that, if the sensibility of the muscles be not a property of the matter of which they are composed, but owing to a superior principle animating them, all the vital and other involuntary motions must ultimately be ascribed to the active power of that principle.

Lastly,

heart contract more slowly, till being by degrees rendered quite insensible, its motion ceases altogether.

BUT,

Lastly, *M. de Haller*, in his treatise *de partibus corporis humani sensibilibus et irritabilibus*, published in vol. 2.

of the *Gottingen Transactions* in 1753, has, by a great many curious experiments, proved, not only that all muscular fibres, and them alone, are endowed with irritability or a power of alternate contraction, but has also shewn, that some muscles and organs are possessed of this power in a greater degree than others. He has further endeavoured to prove, that the irritability of the muscles is independent of the nervous power, and has no connexion with sensibility, but is owing to the glutinous matter of the muscular fibres.

From what has been said, together with the short history of irritability given by *M. de Haller*, (*Aët. Gotting.* vol. 2. p. 154, &c.) it appears, that the contractile power of stimulated muscles has been long known to physicians, tho' within these few years past, it has been made the subject of more particular inquiry.

After the mention made here of *M. de Haller's* history of irritability, and of the result of his own experiments on this subject, I did not expect that he would have complained of my want of equity, in not taking notice of what he had said on those heads, or have

BUT, as *M. de Haller*, who allows that *opium* destroys the irritability of the stomach, intestines and other muscles, denies it to have any power over the heart,\* and seems to call in question those experiments of mine which shew, that *opium*, injected into the stomach and guts of frogs, renders the motion of the heart much slower than usual, and at last puts a final stop to it†; I thought it necessary to endeavour to clear up this matter by some farther experiments, which I shall here briefly relate.

(α) *June* 5th 1755, at 18 minutes past four in the afternoon, I injected a turbid solution of half an ounce of *opium* in eight ounces of water, into the stomach and guts of a frog; and, as it squirted out most of

of  
I never yet  
have alledged, that I had purposely omitted doing him this piece of justice, lest it should have weakened a censure which I intended against him, as having assumed to himself the discovery of the irritable power of muscular fibres. See *Memoires sur les parties sensibles*, &c. vol. iv. p. 124. & 125.

\* *Act. Gotting.* vol. 2. p. 147, 154. & 157.

† *Essay on vital motions*, p. 370, &c.

of the solution injected by the *anus*, I threw in some more in its place. At 24 minutes past five the same evening, I opened this frog, and observed the heart beating very slowly, not above seven times in a minute; when it was touched with the point of a pair of scissars, it renewed its motion faster for two or three pulsations; after which it became as slow as before. The other muscles of this frog were not at this time brought into contraction by pricking or tearing their fibres.

( $\beta$ ) I laid open the whole *abdomen* and *thorax* of a frog; and, at 28 minutes past seven in the morning, immersed it in a turbid solution of *opium*, viz. the same that was made use of in the preceeding and following experiments. At forty minutes after seven, I turned the frog on its back, and observed its heart beating between ten and eleven times in a minute. Having laid it again on its belly, that it might be more exposed to the action of the *opium*; at forty eight minutes past seven, I turned it again on its back, and observing the heart without motion



motion, I opened the *pericardium*; which producing no effect, I cut the heart out of the body, and laid it on a plate, when it contracted twice or thrice, and never after moved, although it was pricked once and again with a pin.

(γ) I cut off a frog's head, and intirely destroyed its spinal marrow by pushing a small probe down through the spine, which occasioned strong convulsions of all the muscles, especially those of the inferior extremities. Ten minutes after this, I opened the *thorax*, and found the heart beating 45 times in a minute. Sixteen minutes after decollation and the destruction of the spinal marrow, it moved 40 times in the minute. After half an hour, it made 36, and, after fifty minutes, only 30 pulsations in the minute, which were now become very small and feeble.

N. B. WHEN the *thorax* of another frog was opened immediately after decollation and the destruction of its spinal marrow, its heart beat 60 times in a minute.

(δ) I

(d) I cut out the heart of a frog, and put it into fountain-water, at twenty three minutes past twelve. After twelve minutes immersion, I took it out of the water, when it beat 20 times in a minute. Having immersed it for five minutes more, it ceased from motion; and when taken out of the water, did not move except when pricked, and then only performed one pulsation.

(e) EIGHT minutes past eleven, I cut out the heart of another frog, and immersed it in fountain-water. 28 minutes after eleven, it continued to move: but its motion, tho' at the rate of eleven pulsations in thirty seconds, was confined to about one third of the heart next its *apex*. Two minutes after this, observing it without any motion, I took it out of the water, and laid it on a table, where it remained at rest, unless when touched. Soon after this, however, it began to move, and, at 25 minutes after immersion, performed 9 pulsations in 63 seconds.

(f) I cut out the heart of a frog, and, at 32 minutes past ten, immersed it in a turbid



sic \*, at my desire, made the following experiment. After making an opening into the cavity of the *abdomen* of a small dog near six months old, he injected by the wound a dram of *opium* dissolved in two ounces and a half of water ; but, before he could stitch up the wound, about an ounce of the solution escaped. Four minutes after making the injection, he laid bare the *thorax*, by dissecting off the teguments which did not seem to give the dog any pain; and could plainly feel the motion of his heart through the *pleura*. It beat 76 times in a minute, but became gradually slower.† Immediately after counting the pulse, Mr *Ramsay* cut the ribs on each side of the *sternum*, which he laid back in the usual way. The heart, which was thus brought into view, appeared quite turgid, and continued in motion about five minutes : during which time it performed only between 60 and

\* Now Fellow of the Royal College of Physicians  
*burgh.*

† This dog's heart, before the injection of the *opium*,  
beat 150. in a minute,



and 65 weak vibrations ; for they were not compleat contractions. While the heart was thus moving, warm spittle was first applied to it, then cold water, and, last of all, oil of vitriol, which shrivelled the parts it touched, almost in the same manner as a hot iron would have done ; but none of them accelerated the heart's vibrations, which became gradually slower, till they ceased altogether.

AGREEABLY to this experiment, we are told by Dr *Alston*, in his learned dissertation on *opium*, that a filtrated solution of this medicine in water, having been injected into the veins of a dog, his pulse, which, when he was first seized with convulsions, was rendered quick and small, became afterwards full and slow\*. And Dr *Kaau Baerbaave* informs us, that in a small dog, which he opened ten hours after he had swallowed three grains of *opium*, the motion of the heart and arteries was very slow†.

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FROM

\* Vid. Medical Essays, vol. 5. p. 1. art. xii.

† Cor. lentissime moyebatur. Motus in arteriis

FROM these experiments it evidently appears, that, as *opium* destroys the sensibility of all the parts of the body, so it deprives the muscles of all power of motion; nor does the heart, in this respect possess any privilege above the other muscles, except that its moving power is not so soon destroyed by *opium* as theirs.

How *M. de Haller* came to be so greatly deceived as to this matter, I cannot pretend to conjecture; since he has not told us in what manner his experiments were made: but, it is not to be doubted, that his candor and love of truth will make him readily acknowledge his mistake, as soon as he shall discover it.

IV. WHEN a viper is pricked with the point of a knife three days after being deprived of its head, heart, and other *viscera*; it moves, not only the muscles whose fibres are touched, but also the other muscles of its body which have no connexion with

(*scil. dura et pia matris*) debilis et valde lentus. Vid. *impet. faciens Hippocrati dictum*. No. 434 & 436.

with those that are stimulated. This indicates either a sympathy between these muscles, which supposes feeling, or some general active principle animating them, which, being affected with a disagreeable sensation by the *stimulus* applied to any one muscle, brings many others into action, in order to avoid what is hurtful to it. In like manner, when a few drops of boiling water fall on one's leg, the muscles which serve to move this member, are instantly and involuntarily brought into contraction, in order to remove it from the offending cause.

A frog, after it has been deprived of its head, will, when touched, often jump and move about for a very considerable time; and it is observable, that, when the toes of its hind feet are any way stimulated, it constantly draws them up to its body; nay, if, when they are in this situation, the toes are again irritated, the legs and feet are not extended, but brought still closer to the body. If one of the legs is pulled down from the body and kept in an extended state;

no sooner are the toes of this foot wounded, than the leg is drawn up to the body as before. Now, if these motions were owing to some property of the insensible matter of which the muscles are composed, Why should not an irritation of the toes be sometimes followed by a contraction of the extensor as well as the flexor muscles of the legs and thighs? But, if we allow them to be owing to the painful sensation in the toes, we shall see that the frog does, in this case, with its limbs, just what a snail does with its horns, when they are roughly touched.

AGAIN, it is very remarkable, that, when the toes of a frog are pricked or otherwise wounded instantly after decollation, there is either no motion produced in the muscles of the legs at all, or a very inconsiderable one. But, if the toes of a frog be touched with one's finger, ten, fifteen or twenty minutes after decollation, the legs and thighs are immediately drawn up to its body; and, if they be at this time wounded, pricked, or cut with a pen-knife,



knife, the muscles, not only of the legs and thighs, but also of the trunk of the body, are, for the most part, strongly contracted, and the animal sometimes moves from one place to another.

Is not the irritation of the toes, immediately after decollation, rendered ineffectual to produce any motion in the muscles of the legs and thighs, by the greater pain occasioned by cutting off the head \*? And are not the muscles of the posterior extremities, as well as of the trunk of the body, brought into action by wounding the toes fifteen or twenty minutes after decollation, because the pain produced by cutting off the head is now so much lessened (perhaps wholly obliterated) as not to prevent the animal from feeling very sensibly when its toes are hurt?

It were to be wished that those who choose to account for the irritability of the muscles, not from their sensibility, but from

\* *Duobus doloribus simul obortis, non in eodem loco, vehementior obscurat alterum. Hippocrat. Aphor. Lib. 2. No. 46.*

from some unknown property of the matter composing them, would, instead of moving objections concerning the seat of the soul, its extension, divisibility and manner of co-existing with the body, favour us, if they can, with some probable explication of the *phenomena* above-mentioned.

V. THAT the motions of irritated muscles are owing to the sensation excited by the *stimulus* applied to them, will appear highly probable, if we consider, that we are, in fact, conscious of many involuntary motions in our bodies proceeding from a particular sensation, either in the organs moved, or in some neighbouring part. This is the case with the motions of the stomach and diaphragm, in vomiting and the hiccup, of the great guts and diaphragm in a *tenesmus*, of the *acceleratores urinæ* in expelling the *semen*, and of the intercostal muscles and diaphragm in sneezing, coughing, and sometimes even in breathing; nay, when, by sudden fear or any great surprize, the heart is set a palpitating, we have a particular feeling in this muscle, partly

partly from the blood, rushing suddenly and in too great quantity into it. More examples might be given, but these may suffice to shew the connexion there is betwixt the sensibility and irritability of the moving organs of our body.

UPON supposition that the motions of irritated muscles did not proceed from any kind of feeling, but from some inanimate cause, their contractions should be all, either regularly alternate, or equable and uninterrupted, like the falling of the leaves of the sensitive plant \*; but we find, that, while most of our muscles are brought, by the action of *stimuli*, into alternate contractions, there are some few which contract uniformly and equably during the time the *stimulus* operates, without any intermissions or alternate relaxations. Of this kind is the contraction of the diaphragm and

\* I have elsewhere shewn by experiments, that the falling of the leaves of the sensitive plant, when touched, does not indicate any kind of feeling, and is no way similar to the alternate contractions of irritated muscles. Essay on vital motions, &c. p. 245.

and abdominal muscles when the *intestinum rectum* is irritated, of the *sphincter pupillæ* while the same degree of light continues to act on the *retina*, and of the muscles of the internal ear as long as the same sound is applied to this organ. Nay, the diaphragm, which is brought into one continued contraction by a *stimulus* affecting the *intestinum rectum*, is agitated with alternate convulsions from an irritation of the left orifice of the stomach, or the olfactory nerves. What account can possibly be given of this, upon supposition that these motions proceed from the *gluten* of the muscular fibres? or what difference can it make to this insensible *glue*, whether the *stimulus* be applied to the nose or *anus*? But, allowing these motions to arise in consequence of an uneasy sensation in the part stimulated, it will immediately appear, that they are performed in such manner as is most effectual to lessen or remove the irritating cause \*.

AGAIN,

\* Vid. An. Essay on the vital and involuntary motions, p. 258, &c.



AGAIN, if the motions of muscles from *stimuli* were not owing to a feeling, How could the convulsive motions of the diaphragm in the hiccup be often immediately stopt by sudden fear, joy, or grief? Why should an irritation of the olfactory nerves become ineffectual to produce sneezing, when some of the muscles of the back or *thorax* are affected with a rheumatism? And why should the convulsive motions of the stomach and diaphragm in vomiting, be frequently interrupted by extraordinary fear, or any very great and sudden surprize? It will be difficult, nay impossible, to give any satisfactory solution of these *phenomena*, if the motions of irritated muscles are supposed to proceed from some unknown property of their *insensible glue*: but they are at once intelligible and clear, upon supposition that they are owing to an uneasy sensation; for as often as this feeling is overpowered by a stronger one in some other part of the body, or when the mind is so suddenly and strongly affected by external objects, as, for a short time, to become almost insensible

sensible of the irritation, the motions owing to it must be lessened or cease.

GRAVITY, magnetism and electricity, are all regular and uniform in their operations; and bespeak nothing of feeling or life in the bodies which are endowed with them, and may therefore be supposed to proceed immediately from material causes; altho' the activity of these causes must be, at last, referred to the great ORIGIN of all power and life in the universe. But the motions of animal bodies from a *stimulus*, are, in many cases, so plainly perceived to flow from an uneasy feeling, their various *phenomena* can be so easily explained upon this supposition, and are so unaccountable on any other, that it is matter of no small wonder to find many learned and ingenious Physiologists using their utmost efforts to overthrow this opinion, and struggling, but in vain, to derive those motions from inanimate matter.

LIFE, sense and proper activity, seem to be inconsistent with the known properties

ties of matter ; wherefore, when we see a system of matter endowed with these, we may, without presumption, conclude, that they are owing, not to the material system alone, but to some active principle animating it. And altho', even upon this supposition, it may be very difficult to account for some of the motions observed in such a system, or in its parts when separated, we cannot hence conclude, that they are not owing to any such power ; but only that our ignorance of the nature of immaterial beings, and of their particular union with, and manner of acting upon bodies, throws a veil of obscurity over these things, which the most enlightened philosopher will never be able to remove.

*M. de Haller*, towards the end of his performance, has thrown out some reflections upon my manner of writing, and the few experiments I had made on dying animals ; which, tho' it were easy to obviate, I shall pass by unnoticed, from a consciousness of their being ill-founded, a dislike

T

of

of introducing any thing personal into a philosophical debate, and a persuasion that my learned adversary himself will not, upon a *cool* review, entirely approve of them.

## APPEN-



# A P P E N D I X.

CONTAINING A

REVIEW of the CONTROVERSY

CONCERNING THE

*Sensibility and Moving Power of the Parts  
of Men and other Animals;*

IN ANSWER TO

*M. de HALLER's late Remarks on these  
Subjects in the 4th Volume of the Me-  
moires sur les parties sensibles et irri-  
tables.*

*Refellere sine pertinacia, et refelli sine iracundia, parati  
sumus.*

CICERO, Disput. Tuscul. lib. 2.

# A P P E N D I X

## REVIEW OF THE ANTIQUITIES

OF THE

ANCIENT AND MODERN HISTORY OF THE

ISLAND OF GREAT BRITAIN

AND

IN A NEW AND

IMPROVED EDITION

OF THE

REMARKS ON THE

REMARKS ON THE

REMARKS ON THE

REMARKS ON THE

REMARKS ON THE

A

REVIEW of the CONTROVERSY  
CONCERNING THE

*Sensibility and Moving Power of the  
Parts of Men and other Animals.*

HAVING, in the notes to the preceding observations, obviated many of *M. de Haller's* remarks of lesser moment, I shall now proceed to give a full answer to those which relate to the chief points in debate between us; and this I shall endeavour to do in such a manner, as to shew that it is not prejudice, but facts, which influence my judgment (a).

BUT, before I enter upon my subject, I must observe, that although my learned ad-

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versary

(a) *Memoires sur les parties sensibles et irritables*,  
tóm. iv. p. 62.

versary has been at great pains, in several places of his writings (a), to represent me as a disciple of *Stahl*, and quite pre-occupied with his opinions; yet the reader will be apt to question the justice of this accusation, after being told, that one of the latest and ablest defenders of the *Stahlian* doctrine (b), has not only attacked my theory of the vital motions, but thinks it less probable than even that of *M. de Haller* and other mechanical physicians. The truth is, I have endeavoured to shew the opinion of *Stahl* and his followers, with regard to the soul's governing and directing the vital motions, to be altogether improbable (c). I have derived all our involuntary motions, whether vital or not, from the irritation of the different organs by various *stimuli*.—I have shewn that irritation only excites motions in our muscles, by affecting them with a disagreeable sensation; and therefore,

(a) *Memoires sur les parties sensibles*, &c. tom. iv. p. 61, 62 and 101.

(b) Dr *Porterfield*, in his treatise on the eye, vol. 2.

(c) Essay on the vital motions of animals, Sect. xi.



therefore, unless sensibility may be a property of matter, that the vital and other involuntary motions must, at last, be referred to the mind, or some sentient principle animating the body (a).

## P A R T I.

## Of Sensibility.

AS I have allowed the tendons, ligaments, *dura mater*, and most other membranes, to be possessed of no more than a very inconsiderable degree of sensibility in a sound state, and have already said enough to shew that the *cornea* and marrow are not insensible, and that the kidneys are much less so than *M. de Haller* has alledged; the only points, of any consequence, with regard to the sensibility of the parts of animals, which remain to be discussed, are 1. Whether those parts, which have little or no feeling in a sound state, may not sometimes acquire a considerable degree of sensibility when they are

(a) Essay on the vital motions, Sect. x. and xi.

are diseased. 2. Whether, from the alledged total insensibility of the tendons, ligaments and membranes, or the invisibility of their nerves, we can reasonably conclude, that they have really no nervous filaments bestowed upon them.

I. WITH regard to the first of these, I had observed, that those parts, which are by all allowed to be sensible, acquire a more acute feeling when they are inflamed; that the parotids, tonsils, lymphatic glands, those of the *Mammæ* and others, which have no acute feeling in a natural state, and are often much swelled without giving any uneasiness, become very sensible when they are inflamed, and occasion severe shooting pains. From analogy, therefore, one would be apt to conclude, that the tendons, ligaments and membranes may, in a diseased state, acquire sometimes a considerable degree of feeling. But as a direct proof of this, I have shewn that in wounds and ulcers, the *tela cellularis* becomes often very sensible; that in morbid cases the *dura mater*, cartilages, ligaments and membranes,

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when laid bare, often granulate, and are sensible of every acrid substance applied to them; that tho' the bones be insensible in a sound state, yet after fractures, or when they exfoliate, there arises from them a soft fleshy substance, which is at first remarkably sensible, but gradually loses its feeling as it grows harder, till, at last, it becomes an insensible *callus* or bone \*.

I have shewn that the tendon of the *biceps* muscle of the arm, after being wounded in blood-letting, has not only been inflamed, but found swelled to ten times its natural size; and that those symptoms, which have been ascribed to a wound of the tendon of this muscle, are never observed after bleeding at the cephalic or jugular veins, although in the last case some small nerves are frequently wounded †.

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\* In the present dispute, the conclusion is the same; altho' the granulation in fractures and exfoliations be supposed with *M. du Hamel* to arise from the *Periosteum*; since *M. de Haller* allows as little sensibility to this membrane as to the bones themselves.

† *M. Muhlmann*. Tho' he has endeavoured to confirm

THE swelling and rigidity brought on the joints, and the calcarious matter collected within their *capsulae*, by frequent attacks of the gout, shew, that this disease has its seat not in the subcutaneous nerves, but in the tendons and ligaments of the articulations. The pain and swelling in consequence of a sprain of the wrist or ankle cannot reasonably be ascribed to the overstretching of the subcutaneous nerves: the weakness of the joint for many weeks or even months, and the pain occasioned by moving it, clearly shew the ligaments and tendons to be the parts which had suffered. The great increase of pain occasioned by the least motion of joints affected with the rheumatism, proves that this disease has also its seat in the tendons and ligaments, which

firm *M. de Haller's* doctrine of the insensibility of the tendons and *periosteum*, yet he acknowledges, that in certain cases the tendons are affected with violent pain. "J'ai remarqué les plus fortes, et les plus insupportables douleurs dans les tendons entrés en putrefaction ou suppures." Vid. *Memoires sur les parties sensibles*, tom. ii. p. 143.



which last can in some cases be perceived to be considerably swelled. Lastly, I have observed, that the *dura* and *pia mater* have been found inflamed and mortified in patients, who have died of a *phrenitis*, and that the headach in the beginning of fevers, has not commonly its seat in the teguments of the skull, but in the *dura* or perhaps the *pia mater*.

IN answer to these, and several other arguments to prove that those parts, which in a sound state have very little feeling, may be rendered very sensible by diseases, *M. de Haller* is pleased to say, that my observations are vague and without precision (a). That to a tendon found insensible, I ought to oppose a tendon which felt the stroke of the lancet, and to a *dura mater* burnt without pain, an instance of convulsions occasioned by burning that membrane. But is not this reasoning plainly evasive? For, since I do not alledge that the tendons or *dura mater*, in a sound state, are possessed

(a) *Memoires sur les parties sensibles*, &c. tom. iv.  
p. 105. 106.

of any other than a very obtuse sensation, all that is incumbent on me, is, to prove, that those and other parts reckoned insensible by *M. de Haller*, are sometimes so changed by diseases, as to be endowed with a painful feeling.

*M. de Haller* has cited from *M. Vari*, an instance of a patient, in whom the *pleura* was found mortified after death, although he had never had any more than an obtuse pain in his *Tborax* \*. Instead of criticising on this case, which is not related with sufficient precision to draw any certain conclusion from it, † I shall oppose to it the following proof, of the sensibility of the *pleura* in a man of 21 years, upon whom *Dr. Middleton*, Physician at New-York, performed the operation of

\* *Vid. memoires sur les parties sensibles, &c. tom. iv. p. 26. & tom. I. p. 427.*

† Physicians are not ignorant, that according to the degree of the disease, and the constitution of the patient, inflammations are attended with very different degrees of pain; and that, after death, small mortifications are sometimes observed, even in the stomach and intestines, altho' no very violent pain has preceeded.

of the *Empyema*. The Doctor, in the account of this patient's case, which he was pleased to transmit to me, has the following paragraph.

" Having dissected down to the *pleura*, the  
 " muscular fibres were thrust aside with  
 " the handle of the scalpel, 'till it was ex-  
 " posed, for about one half inch in length ;  
 " it seemed of a pale whitish colour, and  
 " sounded somewhat like a drum ; the edge of  
 " the knife being now moved very cautiously  
 " and lightly along the exposed *pleura*, we  
 " observed that the patient changed the stile  
 " of his complaining, and cried out with  
 " some vehemence that the pain now went  
 " to his heart. I immediately recollected  
 " the dispute between Dr *Haller* and Dr  
 " *Whytt* relating to the sensibility or insensi-  
 " bility of membranes, and, before the *pleu-*  
 " *ra* was yet penetrated, I asked him if the  
 " cutting now gave him more pain than  
 " before ; he answered very readily in his  
 " way, Oh yes, it some way goes to my ve-  
 " ry heart".

THE *pleura* seems to have been much  
 more sensible in this patient, than it is ever

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in a natural state, and altho' it did not appear red, yet from the collection of *pus* in the *thorax*, it could not fail to be diseased, and probably, in some degree, inflamed.

I have formerly mentioned the bad consequences observed by my worthy colleague Dr *Monro senr.* after piercing the capsular ligament of the knee joint, to let out a watery fluid which had been collected there; and have now to add, that the industrious Dr *Reimarus* has lately given a particular account of three patients, in whom the piercing or wounding of this ligament was followed, after some days, with violent pain, swelling and fever \*. My learned friend Dr *Simson* long before, not only observed the same symptoms, in a patient whose case he describes, but tells us that the most acute pain was felt, when the distended *capsula* of the knee joint was cut †. Nay even the case mentioned by Mr *Warner* is a proof, that the ligaments of the joints are not insensible in a morbid

\* Dissert. de tumore ligamentorum circa articulos p. 15. & 16.

\* Edinburgh Medical Essays, vol. iv. art. 20.



morbid state ; for tho' the patient had a perfect recovery in about 12 weeks, yet he was much pained, not only for several hours after the operation, but also at other times\*.

BUT to these, and indeed every other instance that can be produced, of pain occasioned by cutting those parts, which *M. de Haller* reckons insensible, he has found a ready answer, by affirming, that in such cases, the pain is not owing to those parts themselves, but to some small nerves passing along their surface†. And particularly he ascribes the sensibility of the cellular membrane below the skin, in an inflamed state, to some small nervous branches passing through it, or to the interior *lamellæ* of the true skin, which have been mistaken for the *rela cellulosa*. But this last observation can no ways account for the pain in those ulcers that are deeper than the skin ; and if the first were true, every part of the

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inflamed

\* Vid. Philosophical transactions, vol. xlix. p. 457. compared with *memoires sur les parties sensibles*, &c. tom. iv. p. 59.

† *Memoires sur les parties*, tom. iv. p. 105, & 106.

inflamed cellular membrane ought to be insensible, except those places where the branches of the nerves traverse it ; whereas every point of a suppurated wound in the cellular substance is acutely sensible, as Surgeons daily observe. But further, whence comes the sensibility of that soft granulated substance, which sometimes arises from the surface of the bones, cartilages and membranes ? It cannot be so much as pretended, that there are any branches of nerves running along the surface of this newly generated substance.

Altho' undoubtedly, the pain occasioned by wounding those parts which are reckoned insensible by *M. de Haller*, may sometimes be owing partly or chiefly, to some small nervous filaments running along their surface, but not belonging to them ; yet to assert that this is always the case, seems to be a mere subterfuge, and is a strong indication of the weakness of that cause, for whose support such an argument is necessary. If the pain which *Dr Middleton's* patient felt, when he touched the *pleura* gently, with the point  
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of his knife, was owing to some nerve running along its surface, why was not as great a pain felt in cutting the skin, *tela cellulosa* and intercostal muscles, which could not well be done, without wounding several small nerves? The Doctor's particular attention, in making this experiment, renders it probable that any considerable nerves running along the surface of the *pleura*, would not have escaped his notice; and if there had been but one branch, it was scarce to be expected, he would have hit upon it, not only when he first gently touched the *pleura*, but also afterwards, when he cut it. Those patients mentioned by Dr *Reimarus*, who suffered so much after piercing the capsular ligament of the knee joint, felt but little when the wound was made, nor did the violent pain come on for several days\*. This, however, could not have been the case, if their bad symptoms had been owing to the cutting or wounding some small branches of nerves. Further, the pain was felt through the whole

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ligament

\* Dissert. de tumo. ligament. p. 15. & 16.

ligament of the joint, and not chiefly or solely in the wounded part, which, however, must have happened, had it proceeded from the nerves \*.

I shall only add, that since it appears from certain observations, that the membranes, tendons, and ligaments are liable to inflammation, it will likewise follow that they are in such a situation sensible. Physicians and Surgeons know that the several parts of the body acquire a more acute feeling by inflammation; that the more sensible any part is, the more apt it is to be inflamed by irritation, and that such *Calli* as are quite insensible are also incapable of inflammation. Indeed, to assert that any part may be inflamed, and yet remain altogether insensible, is a paradox that will hardly go down with any one, who has been much conversant in practice.

BUT it will still be objected to what has been said, how can we suppose those parts to be endowed, in any state, with any degree of feeling, which are destitute of nerves,

\* Dissert. de tumor. ligament. p. 17.



nerves, and which numberless experiments on living animals have shewn to be insensible? In answer to which, it is proper to observe, that *M. de Haller's* experiments do not demonstrate either the tendons, ligaments or membranes to be *altogether* insensible, any more than the liver, spleen, kidneys and ureters; for when these last parts were pricked or cut, the animals shewed no signs of pain †. If therefore we know, from undoubted observations on men, that the ureters, kidneys, &c. are not destitute of feeling, it will follow that the tendons, ligaments and other parts reckoned wholly insensible by *M. de Haller*, may, notwithstanding his experiments, be also endowed with some small degree of feeling, and consequently may, like other parts of the body, acquire a greater sensibility, when they come to be in an inflamed state.

If, indeed, it could be proved that the tendons, ligaments and membranes are really destitute of nerves, *M. de Haller's* conclusion

\* *Memoires sur les parties sensibles*, &c. vol. iv. p. 106.

† *Ag. Gotting.* tom. 2. p. 131 & *memoires sur les parties sensibles*, tom. iv. p. 33.

clusion would certainly follow, *viz.* that in no case could they become the seat of painful diseases. But this leads me to the second point, which I proposed to consider, *viz.* whether, from the alledged insensibility of any part, or the invisibility of its nerves, we can justly conclude that it has none.

II. *M. deHaller* is of opinion that the tendons, ligaments, cartilages and membranes, have no nerves. First, Because if they were furnished with nerves they could not be insensible; and 2dly because anatomists have not been able to trace their nerves, either with the dissecting knife, or to discover them by the help of the microscope\*.

(1.) With regard to the first of these arguments, we have already shewn that *M. de Haller's* experiments only prove the parts in question to be destitute of any painful feeling, but not that they are altogether insensible; and it is most certain that a part may be furnished with nerves, and yet have little or no sensibility. The quick feeling of that soft vascular substance, which

\* *Memoires sur les parties sensibles, &c.* tom. iv. p 28. &c.

which arises from bones after exfoliation, and also from cartilages, is a convincing proof, that these parts, tho' in a natural state the most insensible of any in the whole body, are not destitute of nerves : And it ought always to be remembered, that altho' the sensibility of the several parts proceeds from their nerves alone, yet, in certain parts, these nerves may be so compressed or otherwise changed, as either to feel very obtusely, or perhaps not at all.

(2.) We cannot conclude with certainty, that a part has no nerves bestowed on it because they are invisible ; for there are many vessels in the human body, whose existence we must acknowledge, tho' they be too small to be discovered by our senses ; and it will hardly be doubted, that the smaller insects are furnished with muscular fibres, whereby they perform their various motions, altho' these instruments are too minute to be discovered even by the help of the microscope.

With regard to the tendons, since it cannot be denied, that many parts, which were  
truly

truly muscular in a *fœtus*, become in an advanced age tendinous, we must conclude, that the tendons, as well as the muscles, are furnished with nerves, altho' the nerves of the former may be so compressed by their hard compacted substance, as in a great measure to be deprived of their sensibility. The inflammation and pain consequent upon opening the *capsule* of the joints is a proof that these parts have also nerves bestowed upon them: And granting that no nerves could be demonstrated to terminate in the *pleura* and *dura mater*, as *M. de Haller* alleges, which, however, is doubtful, since the accurate *Winslow* describes them; yet the inflammations with which those membranes are often affected, and that sensible granulated substance which sometimes arises from them, are a sufficient proof, that they are not without nerves.

Altho' anatomists are unable to demonstrate the nerves of the marrow and the *cornea*, or conjunctive covering it, yet their sensibility is a proof that they are furnished with nervous filaments. *M. de Haller*, who  
supposes



supposes the *cornea* to be a continuation of the *epidermis*, or of the nature of the nails, ascribes the pain felt upon touching the fore part of the eye, to some small nerves that run between the conjunctive and *cornea* (a). But not to mention, that the vascular structure of these membranes shew them to be essentially different from either the cuticle\* or the nails, I may venture to affirm, that no anatomist has yet discovered any branches of nerves running between the conjunctive and *cornea*; where, indeed, they could be of no use; but, if they happened to pass opposite to the pupil, would disturb the distinct vision of objects. The existence, however, of such nerves, altho' it were granted, would by no means account for the conjunctive being affected by the slightest touch, and by *stimuli* which  
raise

(a) *Memoires sur les parties sensibles*, &c. tom. iv. p. 59. and 108.

\* Since the *cornea* is covered by the conjunctive, it is not easy to conceive how it can be a continuation of the cuticle which is the most external tegument of the body.

raise no pain in the lips or *fauces*, because it is the extremities of the nerves that feel most acutely, and not their sides which are defended by the coats which surround their medullary substance. Further, if the pain occasioned by touching the eye, were owing, not to the conjunctive which covers it, but to the nerves running between it and the *cornea*, how should it happen, that the slightest touch gives it sensible pain, whereas, when the eye-lids are shut, a considerable pressure on the forepart of the eye gives little or no uneasiness?

Lastly, since the small arteries are allowed by *M. de Haller* to have nerves (*a*), which indeed their muscular structure supposes, it will follow that the tendons, ligaments and membranes cannot be altogether destitute of nerves; for those parts are, in a great measure, a contexture of vessels that carry either red blood or finer fluids; and no anatomist, however subtile, can be certain, that the invisible nervous filaments, entering

(a) *Memoires sur les parties sensibles*, &c. tom. iv. p. 87.

entering those parts along with their arteries or veins, may not be partly bestowed on that portion of their substance which is not vascular. But be this as it will, if the tendons, ligaments and membranes consist partly of vessels that are endowed with nerves, and are not altogether insensible, they may certainly, when inflamed, acquire a greater degree of sensibility.

## P A R T. II.

## Of Irritability.

HAVING formerly shewn that we have no reason to doubt of the irritability of the small arteries and several other parts, altho' *M. de Haller's* experiments do not discover any such power in them, I shall, passing by some points of less moment, proceed to inquire particularly, whether the power of contraction observable in such muscles as are irritated depends upon the

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nerves,

nerves, and is connected with sensibility, or whether it may not be a property of that glutinous matter which partly composes the muscular fibres.

In an essay on the vital and other involuntary motions of animals, published in 1751, I endeavoured to prove that the motions excited in the muscles by irritation, are owing to a disagreeable feeling in them or their nerves; but *M. de Haller*, after having made a variety of experiments on living animals, concludes, that irritability is an innate property of the muscular fibres, or rather of their glutinous substance, and denies that it depends on the nerves, or has any connection with sensibility\*. 1. Because the most sensible parts, such as the skin and nerves, are not irritable†. 2. Because the irritability of the muscles is not observed to be in proportion to their sensibility‡; and, 3. Because parts destitute of feeling are irritable§. After giving a particular

\* Vid. æt. Gottingens. vol. 2. published in 1753.

† Aët. Gotting. vol. 2. p. 134.

‡ Ibid. p. 136.

§ Ibid. p. 134.



particular answer to each of these arguments †, I endeavoured to shew, by many examples, that the irritability of the muscles or muscular organs is always increased, when, from any cause, their sensibility is rendered greater—that in different ages, the irritability of the muscles bears some proportion to their sensibility—that when the stomach is disordered, the heart is often rendered more irritable, merely from sympathy, and in a *nephritis* the stomach becomes so sensible that it can scarcely bear any *stimulus*. I have proved, by various instances, that the irritability of the muscles is lessened, or even destroyed by cold and by *opium*, which also lessen or destroy sensibility. I have mentioned some decisive experiments on dying animals, which demonstrate the connection between sensibility and irritability; and lastly, I have shewn that the *phenomena* of many of the involuntary motions in men, are not to be accounted for, without supposing them to proceed from feeling\*.

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Now,

\* Vid. Physiolog. Essays, edit. 1. p. 151—188.

† Vid. Physiolog. Essays, edit. 1. p. 188—253.

Now, in what manner has *M. de Haller* answered this chain of argument, which I had brought to prove the connection between irritability and sensibility. He tells us, that the nerves, although the most sensible parts of the body, are not irritable, and that sensibility is as the number and bareness of the nerves, while irritability is as the number of muscular fibres exposed to the irritating cause, and therefore these two powers are in no proportion to each other. He observes, that insects, which have neither a brain nor nerves, are nevertheless endowed with irritability. He mentions a few detached *phenomena*, some of which may at first view seem to make for him, while others are no ways applicable to the point in question; and insists, as he had formerly done, that since the muscles continue to be irritable for sometime after their nerves are tied or cut, this property certainly subsists after the power of the nerves is altogether destroyed, and therefore can have no dependence on sensibility†.

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† Vid. *Memoires sur les parties sensibles et irritables*, tom. iv p. 92. & 93. & p. 118—124.

As this last argument is the only one of any consequence, which *M. de Haller* has brought against the connection, which I have endeavoured to prove between irritability and sensibility; I shall begin with considering it particularly; and if I shall be able to make it evident by the plainest facts, and the fairest analogy, that the motions of muscles whose nerves are cut, neither shew that irritability is independent of the nerves, nor even that it has no connection with sensibility, I shall find little difficulty in refusing every thing else that my learned adversary has advanced, either in favour of his own system, or in opposition to mine.

1. Emetics produce convulsive contractions of the stomach, not by irritating its muscular fibres, but by affecting its nerves with a disagreeable sensation. The *semen* does not excite the *acceleratores urinae* into alternate motions by acting on their fibres, but by stimulating the nerves of the *urethra*. The same is true of the natural *stimuli* which act on the intestines, heart, and other muscular organs; nor can one instance be produced, in the human

human body, of any vital or involuntary motion, that is owing to an irritation of the muscular fibres of the organ itself. If then, in a natural state, every instance of the motion of muscles from irritation, is owing to a *stimulus* acting on their nerves, and not on their fibres, is it not reasonable to conclude, that the contractile power or irritability of the muscles depends upon their nerves, and is connected with sensibility? But to this conclusion it will be objected, that, in many animals, the muscles continue to preserve their power of motion for a considerable time after their nerves are tied or cut.

After separating one of the thighs of a frog from its body, the muscles, when laid bare and irritated, are not brought into any strong contraction; but their fibres, or sometimes only a few of them, are agitated with a weak tremulous motion. But when the extremity of the crural nerve, where it was cut, is irritated, the muscles of the thigh and leg are strongly convulsed; and after this part of the nerve has been rendered insensible by repeated



repeated irritations, and lost its power over the muscles, if it be pricked a little below, the member is convulsed as before: And in this manner, by proceeding gradually downwards with the irritation of the nerve, convulsive motions of the muscles were renewed and continued for above a quarter of an hour by Dr Oeder of Copenhagen\*.

AGREEABLY to this, when the Phrenic nerve, in a dog, is compressed or cut, and pricked below this, the diaphragm is contracted in the same manner, as if the communication of its nerve with the brain had not been interrupted; and by descending downwards with the irritation, the motion of the diaphragm may be renewed and continued for some time†.

Do not these experiments clearly prove, that the nerves, after being separated from the brain and spinal marrow, retain their power over the muscles: And if, after destroying, by frequent irritations, the superior

\* *Memoires sur les parties sensibles*, &c. tom. 2. p. 61.

† *Memoires sur les parties sensibles*, tom. 1. exp. 222. & 225.

rior part of any nerve, the muscle, to which it belongs, can be still convulsed by proceeding downwards with the irritation, till we arrive at the muscle itself, upon whose fibres a *stimulus* has much less effect, than it had on any part of the nerve; does it not clearly follow, that the irritability of the muscles depends on their nerves, that when the fibres of any muscle are stimulated, the contraction which follows, does not proceed from any active power, or innate property in these fibres themselves, but from the irritation of the small nervous filaments which terminate in them; and that a *stimulus* applied to a muscle after its separation from the body produces only a very feeble contraction, while an irritation of its nerve occasions strong convulsions, because in the former case its nerves do not suffer so much as in the latter, nor are so many of them exposed to the stimulating cause?

If greater motions were excited in the muscles by an irritation of their fibres, than of the nerves going to them, or if the muscular fibres continued to be affected by

*stimuli*

*stimuli*, after their nerves had lost their power altogether, it might fairly be concluded, that their irritability was in a great measure independent of the nerves, and owing to the fibres themselves; but we see that the contrary of all this is true \*. And indeed, whatever deprives the nerves wholly of their powers, renders the muscles, at the same time, altogether incapable of motion. Thus tho' frogs live and can move their limbs and body for several hours after their head is cut off, yet in less than an hour, after injecting a solution of *opium* in water into the stomach and intestines of these animals, their muscles are neither convulsed by the strongest *stimuli* applied to their fibres or nerves, nor even by an irritation of the spinal marrow †.

THE plainest facts, therefore, prove that the irritability of the muscles depends on their

\* I have observed in frogs, to whom I had given *opium*, that a probe pushed into the spinal marrow produced a feeble contraction of the fore-legs, after pricking and cutting their muscles had failed to excite any motion.

† Essay on the vital motions, &c. p. 372.

their nerves, and that the nerves preserve their power over the muscles, for a considerable time after they are separated from the brain or spinal marrow ; and it is really matter of wonder, that these points should have ever admitted of dispute, after the numerous experiments that have been lately made to discover the nature and source of that power which puts the muscles in motion \*.

## THE

\* *“ Pour exciter du mouvement dans les muscles par  
 “ l’irritation des nerfs, il n’est pas nécessaire, que  
 “ ce nerf ait conservé sa continuité avec le cerveau, ni a-  
 “ vec la moëlle de l’épine. Car l’irritation d’un nerf  
 “ entièrement séparé de la moëlle de l’épine ou du cer-  
 “ veau, produit les mêmes contractions dans le muscle, que  
 “ celle d’un nerf, dont la continuité avec ces parties est  
 “ conservée. Mémoires sur la nature irritable. tom.  
 1. p. 237. & 238.*

*“ Ces expériences confirment, ce que j’ai dit un peu  
 “ plus haut, (p. 238.) Qu’on comprime qu’on lie, qu’on  
 “ coupe le nerf d’un muscle, & qu’on intercepte tout le  
 “ commerce, qu’il avoit avec le cerveau : qu’on irrite  
 “ ce nerf, pourvu qu’il soit encore frais & humide, ces ir-  
 “ ritations produisent dans les muscles auquel ce nerf  
 “ aboutit les mêmes mouvemens, qu’elles auroient pro-  
 “ duit,*



THE nerves are not to be considered merely as the excretory ducts of the brain and spinal marrow, but as real continuations or productions of their medullary substance, which

*“dûit, si sa continuité avec le cerveau étoient entière.*

*“Ce theoreme ayant été prouvé pour les nerfs, qui obéis-*

*sent à la volonté, l'est ici pour les nerfs vitaux.*

*“Ibid. p. 245, & 246”.*

Could any one have imagined that *M. de Haller*, after the above acknowledgment, would have concluded that the motions of those muscles, which he irritated after he had cut their nerves, were performed without the assistance of these nerves (*a*)? And that he could have been so blinded as not to see, that irritability, or the second force which he ascribes to the muscles, is owing to the small nerves which terminate in them, and which, like their larger branches, preserve their power for a considerable time after they are cut or tied? *M. de Haller* allows, that the more violent motions of the muscles are produced by irritating their nerves, and that *opium* which destroys sensibility deprives the nerves of this power (*b*). Is it not, then, highly probable that the weaker convulsions of the muscles excited by an irritation of their fibres, which are of a similar nature with those that are stronger, (*c*) and are also

*opium,*

(*a*) *Memoires sur la nature irritable, tom. 2. exper. 248.*

(*b*) *Ibid. p. 237. & 256.*

(*c*) *Ibid. p. 256. & 257.*

which are endowed with certain powers that they retain in a great measure, even after being divided from their origin : And if the several parts of a *polypus* preserve a power of motion and life ; and become capable of forming new animals after they are divided, why may not the *medulla oblongata*, spinal marrow and nerves of the larger animals, retain their powers, in a lesser degree, for a short time after they are separated from each other ?

2. Having made it sufficiently evident that irritability depends on the nerves, we come next to consider whether the motions excited by *stimuli* in those muscles whose nerves are tied or cut, be a proof that irritability has no connection with sensibility \*.

AND here it is obvious to observe, that, if the convulsive motions occasioned by irritating

destroyed by *opium* (a), proceed from the power of those small invisible nervous filaments with which they are furnished, and not from any power or property in the muscular fibres independent of their nerves ?

(a) Vid. Essay on vital motions, &c. p. 372, &c.

\*Vid. *memoires sur les parties sensibles*, &c. tom. iv. p. 119.

tating the medullary part of the brain, or the *medulla oblongata* in dying animals, are allowed to be a proof of their sensibility; and the like motions excited by stimulating the *medulla spinalis*, after the head is cut off, shew that it still continues to feel \*; it will necessarily follow, that the convulsions produced in the muscles by irritating their nerves after they are cut, or below the ligature after they are tied, are equally strong indications

\* That the convulsive motions excited in the muscles by irritating the *medulla oblongata* or *spinalis* are owing, not to the mechanical propulsion of any fluid through the nerves proceeding from them, but to the sensibility of those parts, can scarce be doubted, since the convulsions occasioned by irritating and breaking down the brain in a dog to whom Dr *Kaau Boerhaave* had given six grains of *opium* were a hundred times less than those which he had been in use to observe in other dogs who had got nothing to lull their senses (a). Further, the effects produced by irritating the *medulla oblongata* or *spinalis* are vastly greater than could be expected from such a cause; thus not only a small irritation with the point of a needle, but acrid substances, which do not act by mechanical impulse, so affect those parts, as to excite strong contractions of the muscles.

(a) *Impet. faciens Hippocrat. dict. No. 435.*

indications of sensibility in these nerves. Nay, *M. de Haller* himself has expressly told us, however unfavourable it may be to his system, that after cutting the spinal marrow of frogs in two, their hind-legs continued to be sensible, and the animals felt the irritation of their nerves. See *Memoires sur la nature sensible*, &c. tom. 1. exp. 201. & 202.

BUT further, unless those motions produced by irritation, were owing to a disagreeable sensation excited in the muscles or their nerves, how comes it that the muscles of a frog's legs and thighs, which are not sensibly moved by pricking its feet or toes immediately upon decollation, should, in ten or fifteen minutes after this, be brought into motion by touching those parts with one's finger, and be strongly convulsed by cutting them with a pen-knife? In this case, does not the greater pain occasioned by cutting off the head, render the irritation of the feet ineffectual, at first, to produce any motion in the muscles of the legs and thighs? and are not these parts moved, afterwards, by a much weaker *stimulus*; because the pain  
of



of decollation is so much abated, as not to prevent the animal from feeling when its toes are touched? This experiment, which I have repeated several times, is related more fully in the first edition of the preceeding observations; and as I looked upon it to be a decisive proof of the connection between sensibility and irritability, I desired that the learned *M. de Haller*, or any of his disciples, would favour me with some probable explanation of it upon their principles. But as my learned adversary, in his answer, has not taken the smallest notice of this experiment, nor attempted, in any manner, to remove its force, I must be allowed to think it now an unanswerable proof, that the motions produced by irritation depend upon the sensibility of the parts.

It is observable, that an irritation of the nerves, which are the most sensible parts, produces the most violent motions in the muscles; and when they are, by being stretched, rendered more susceptible of pain, still greater convulsions are occasioned by pricking them: The irritation of the mus-

cular fibres, which are less sensible than the nerves, excites only a weaker contraction; and *stimuli* applied to the tendons, which are in a natural state destitute of any painful feeling, produce no motion at all, altho' these tendons be really a continuation of the muscular fibres. These *phenomena* will not be easily explained, except upon the supposition, that the motions excited by irritation are owing to an uneasy feeling.

BUT this will appear still more evident, if we consider that those motions, which are occasioned by *stimuli*, acting, not on the organs moved, but on distant parts, cannot possibly be owing to any change made on the glutinous matter of the muscles, but proceed from that *sympathy*, which prevails in the nervous system; and must be ascribed to an uneasy sensation in the part irritated, since all *consent* supposes feeling, and is indeed inexplicable upon any other principle.

THUS the contraction of the pupil from light affecting the *retina*; the convulsive motions of the stomach and diaphragm, which attend an inflammation of the kidneys,

or

or are occasioned by a stone irritating the ureters ; the accelerated motion or palpitation of the heart from a disorder in the stomach ; the convulsion of the diaphragm from an irritation of the nose, and its continued contraction when the *intestinum rectum* or bladder are stimulated.

THESE, and other sympathetic motions, which might be mentioned, are all owing to an uneasy feeling in the parts irritated, whence certain organs, with which they have a particular sympathy, by means of the nerves, are either excited into convulsive contractions, or rendered so irritable, as to be much more easily affected by *stimuli*, than usual. If then, motions occasioned by the irritation of a distant part, proceed from feeling, it will appear no less probable that those contractions, which are excited in any muscular organ by irritating its fibres or nerves, are owing to their being hurt by the *stimulus*.

IT is observable, that all sympathetic motions cease as soon as the brain and spinal marrow are destroyed, or the communica-

tion between them and the nerves is cut off; the reason of which is, that the nerves have no sympathy but at their origin: \* while, on the other hand, the contractions produced by an immediate irritation of the parts moved, or of their nerves, continue for some time, because the nerves do not at once lose their powers after being separated from the brain and spinal marrow.

If it shall be asked, How can a part feel when it has no connection with the brain? I answer, That I know not how it feels either when it is connected with the brain, or separated from it; but undoubted experiments shew that it feels in both cases †; for

\* The truth of this proposition, and that sympathy is not owing to any communication or connection of the nerves in their course from the brain to the several parts of the body, I have endeavoured to prove in some observations on the sympathy of the nerves, which I propose soon to lay before the public.

† *M. de Haller* himself acknowledges that the hinder limbs of a frog feel after its spinal marrow is cut in two. *Memoire sur la nature irritable*, tom. i. exp. 202.



for we have seen that the nerves, when irritated, exhibit the same marks of sensation after being tied or cut, as they do in an entire state : And here I am naturally led to consider the metaphysical arguments, which *M. de Haller* has brought to prove that irritability has no connection with sensibility ; but since, in his late remarks \*, he has done little more than to renew the same objections to which I had formerly given a sufficient answer †, and as a physical question must be determined by facts and observations, and not by metaphysical reasoning, I shall only suggest, that when my learned adversary confines the soul to the brain, he has certainly forgot, that a frog lives, moves, and is sensible, thirty hours, a viper three days, and a tortoise six months, after the loss of the brain ‡. When he argues, that if the whole nervous system, or the whole body be animated,

\* *Memoires sur les parties sensibles, &c. tom. iv. p. 101. &c.*

† Vid. *Physiological Essays*, edit. 1. p. 168, &c.

‡ Vid. *Physiological Essays*, edit. 1. p. 168. &c. and *Essay on the vital motions*, p. 385, and 386.

mated, we ought to be sensible of losing a portion of our soul when an arm or a leg is cut off\*; he does not seem to have been aware, that an immaterial substance cannot, like the body, be divided by the anatomical knife, and that the indivisibility of the soul does not depend on the unity of that body which it animates, but on its own particular nature. Lastly, When he concludes that a member, whose nerves are tied or cut, or which is separated from the body, can have no kind of sensibility, because, when it is pricked, or otherwise wounded, the person, to whom it belongs, feels not the smallest pain†, he had certainly not attended to what I had suggested to shew, that it is only in the brain that the soul reasons, remembers, and is conscious of the feelings of the different parts of the body‡. When a nerve or a muscle is irritated after its communication with the brain is cut off, although there be some kind of feeling in the part itself, yet the animal has

\* *Memoires sur les parties sensibles, &c. tom. iv. p. 121.*

† *Ibid. p. 119, and 120.*

‡ *Physiological Essays, edit. 1. p. 169. &c.*

has no consciousness of this, because the common *sensorium* is not affected by it.

THAT learned Philosopher *Colwallader Colden*. Esquire, Governor of New York, seems to have understood this matter well, and has set it in such a light as may be sufficient to answer all *M. de Haller's* metaphysical difficulties. In a letter to me, dated at New-York, April 15. 1760, he expresses himself as follows :——“ The mind is not  
“ confined to any particular part of the  
“ body ; for nothing can act where it is  
“ not. But we are only conscious of those  
“ sensations which are communicated, by the  
“ nerves, to the common *sensorium* ; for  
“ otherwise the action of any one part, has  
“ no relation to the whole system ; we are  
“ not otherwise conscious that we feel. In  
“ this common reference of every part to  
“ the whole, personality or sameness and consciousness consist. So likewise it seems  
“ probable, that all operations of the mind,  
“ of which we are conscious, or all actions  
“ of the will arise in the common *sensorium*.  
“ The mind may have different perceptions  
“ of

“ of the actions of bodies or of matter in  
 “ the part on which any body immediately  
 “ acts, from what it has of the same action  
 “ communicated to the common *sensorium*;  
 “ but we are only conscious of the last: and  
 “ so we may not be conscious of the opera-  
 “ tions of the mind, when its action does  
 “ not proceed from the common *senso-*  
 “ *rium*.”

BUT to return,

WHETHER the irritability of the muscles  
 depends on their nerves, and is connected  
 with sensibility, or not, is a point that must  
 be determined by experiments alone; and  
 for this purpose it is sufficient, if we can  
 prove that the nerves are endowed with  
 feeling and a power of moving the muscles;  
 and that, after they are tied or cut, they re-  
 tain these powers, in some degree and for  
 some time. It is altogether unnecessary to  
 inquire, whether those powers of the nerves  
 be owing to the particular disposition and  
 arrangement of the matter of which they  
 are composed, or to some immaterial prin-  
 ciple animating them. This is a question  
 entirely



entirely metaphysical and no ways connected with the dependence of irritability on the nervous power; and whichever way it may be decided, the arguments will remain in full force, which I have used to prove that the motions produced in the muscles by irritation, are owing to the nervous power, and connected with sensibility.

I have formerly offered it as my opinion, that not only the sensibility of the several parts, but that power of motion, which the muscles possess, must be referred, at last, to that living principle, which many of the wisest philosophers in all ages have supposed to animate the human body: But I will venture to say, that, leaving the soul out of the question, many who are not *materialists*, will think it as reasonable to ascribe feeling to the nerves, as to attribute the active power of irritability to the glue which partly composes the muscular fibres; for dead matter seems to be as little capable of real activity, as of sensibility.

III. It is argued, that the irritable power of the muscles appears to be independent of  
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the nerves, because many insects are possessed of it in a remarkable degree, altho' they have neither a brain nor nerves \*.

To this argument I had answered before, that even the smallest animals might have nerves or something analogous to them, though, on account of their exility, they are not to be discovered even by the best microscopes. But now I am told, that, since I affirm insects have nerves, it is incumbent on me to demonstrate those parts which our senses disavow †. Are we then to deny the existence of every thing, which does not fall under the cognizance of our senses, and reject all reasoning concerning the structure of animals, from analogy? Will *M. de Haller* think it a sufficient answer to the arguments he has brought to prove, that the medullary fibres of the brain and the nerves are hollow tubes and filled with a fluid ‡, to say, that, since he affirms the

\* *Memoires sur les parties sensibles, &c.* tom. iv. p. 92, 122. and 123.

† *Ibid.* p. 122.

‡ *Prinæ Linæ Physiolog.* 387, &c.

the nerves to be hollow, it is incumbent on him to demonstrate these cavities which our senses disavow?

As a single nervous fibre is too subtile to be distinguished, even in the largest animals, by the best microscopes, why may not insects be endowed with nerves, altho' too small to be discovered by our senses? The ingenious Mr *Lyonet* of the *Hague* has, as I am informed, by the help of the microscope, discovered and delineated not only the *medulla spinalis* in the caterpillar that feeds on the willows, but the nerves sent out from it to the several parts of that animal; and it is highly probable, that many other insects who have no head or brain, and have been thought to want nerves, may really be furnished with them, altho', on account of their smallness, they may forever escape our most industrious researches.

BUT supposing, what is by no means probable, that most insects were really destitute of nerves; would it thence follow, contrary to the clearest experiments, that the muscles of larger animals do not owe their power

of motion, as well as sensation, to their nerves? Because insects, in which we can observe no brain or nerves, are endowed with feeling, and undoubtedly perform voluntary motion, are we to conclude, that sensibility and voluntary motion in men do not depend on the brain and nerves?

It is observable, that *M. de Haller* only concludes, from the invisibility of the nerves in *Polypi* and other microscopic insects, that there may be *muscular motion* where there are *no nerves* \*, but if he had reasoned fairly, he should have also added, *nor muscles*; for in those insects we can as little demonstrate a muscular as a nervous structure. This, however, was wisely kept out of view, as the absurdity of ascribing muscular motion to animals destitute of muscular fibres, would have been rather too glaring, and must have exposed the weakness and fallacy of this whole argument.

THE truth of the matter is, that many insects, in which we can neither discover  
muscles

\* *Memoires sur les parties sensibles, &c. tom. iv. p. 123.*



muscles nor nerves, appear, as far as we can judge from experiments and observation, to be endowed not only with a power of motion, but also with sensation; the proper conclusion from which is, not that the muscles and nerves in men and other animals are unnecessary to motion and sensation; but that those insects are either possessed of these instruments, altho' on account of their smallness they be invisible, or that the great AUTHOR of nature has endowed almost every part of their bodies with the powers of sensation and motion, although he has neither given them nerves nor muscular fibres like those of the larger animals..

IV. In order to shew the connection between irritability and sensibility, I had observed that the irritability of the muscles or muscular organs in the human body, was observed to bear a proportion to their sensibility \*. To this it has been answered, that sensibility is proportioned to the num-

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\* *Physiological Essays*, edit. 1. p. 189..

ber and barrenness of the nerves, while irritability is in proportion to the number of fibres exposed to the irritating cause †. The observation is most certainly true, but, like many others that have been brought against me, is nothing to the purpose. The nerves and the skin, altho' the most sensible parts of the body, are not irritable like the muscles, because they are not, by their structure, fitted for motion; the number, therefore, and barrenness of the nerves bestowed on any part, will not make it irritable, unless it be of a muscular nature: And surely no one, who had any tolerable knowledge of the human body, could ever imagine, that the moving power of its different parts should be proportionable to their sensibility *alone*, whatever difference there might be in their structure or other circumstances. But where the same muscular structure takes place, and the same number of fibres are affected by the stimulating cause, there irritability will be found  
always

† *Memoires sur les parties sensibles, &c.* tom. iv.  
p. 92.

always proportional to sensibility. The effects of inflammation and other examples formerly produced \*, clearly prove, that the irritability of the muscles and organs, in the same person, is at different times, *ceteris paribus*, proportioned to their sensibility: And the frequent palpitations of the heart, from slight causes, in those women who are possessed of a very delicate nervous system, as well as many other facts that might be mentioned, shew that the irritability of the heart in different persons, is proportional to its sensibility. In like manner, are not the *intestinum rectum* and the bladder of urine rendered impatient almost of any *stimulus*, when their sensibility is increased by their being inflamed, excoriated, or ulcerated?

BUT it has been said, that the stomach, tho' less irritable, is nevertheless more sensible than the heart †. The nerves on the internal surface of the stomach, have a very peculiar feeling, whereby they are dis-

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\* Vid. Physiological Essays, edit. 1: p. 189, &c.

† Act. Gotting. vol. 2. p. 136.

agreeably affected by many substances that neither offend the tongue, *fauces*, nor even the eyes : but, on the other hand, they can bear the touch of brandy, vinegar, and hot spices, with much less uneasiness than those parts, or than the skin when deprived of the cuticles. The sensibility of the nerves on the inside of the stomach is, therefore, of a peculiar kind, and cannot be properly compared with the feeling of the nerves in the other organs. But supposing, that the heart were less sensible than the internal surface of the stomach, yet it is to be considered, that these organs are not, in other respects, alike ; and there can be no doubt, that of organs, which are only in part muscular, the most sensible may be the least irritable. The muscular fibres in the stomach are vastly fewer in number, and may, for any thing we know, be also less sensible than those of the heart. The small degree of sensibility which the muscular fibres of the stomach possess, when compared with the delicate and peculiar feeling of its internal surface, seems to be  
the



the reason why, in living animals whose *abdomen* is opened, pricking the stomach with a needle or the point of a knife does not produce any such convulsive motions of its muscular coat, as are occasioned by the much weaker irritation of its inner surface by emetics, disagreeable aliments, and other slight causes.

THE muscular fibres, as far as can be gathered from experiments on brutes, or observations on men, are only endowed with a middle kind of sensibility, and have a less acute feeling than the nerves, or even than the skin and several other parts \*. It is no ways inconsistent, therefore, with the great irritability of the heart, that it may be less sensible than the skin when deprived of the *cuticle*, or even than the internal surface of the stomach, or that animals do not shew signs of great pain when it is irritated †. All that

\* Vid. Brocklesby in Philosoph. Transact. vol. xlix. p. 243.

† After opening the *thorax*, it is not to be expected that animals can feel additional pain from wounding their

that is required to its greater irritability, is, that it abound more in muscular fibres, and be possessed of a sensibility, at least equal; if not superior, to that of the other muscles. The very numerous muscular fibres of the heart will not be denied, and there is no experiment or observation that shews these fibres, in a natural state, to be less sensible; than those of the other muscles\*. Indeed the heart's preserving life so long, and its being so sensible of all kinds of *stimuli*, are strong presumptions of its delicate feeling, and that its nerves as well as those of the intestines may, perhaps, be so constituted as to preserve their powers longer, after being separated

their heart, unless it were much more sensible than the parts which have been cut before. Besides, we cannot judge with certainty of the feelings of a dying animal.

\* The external surface of the heart and intestines is rendered less sensible than it would otherwise be, by being covered with membranes that are possessed of very little feeling: and hence that woman, whose intestines were handled by *Peyers*, complained of no pain. *Pa-reg. anat. exercitat. i. cap. iv.*

pared from the brain and spinal marrow, than those of the other muscles.

It has been justly observed by *M. de Haller*, that the heart, in dying animals, is much more affected by the gentle *stimulus* of warm water pushed into its ventricles, than by applying the most acrid liquors to its external surface, or even pricking it with the point of a knife † : And we know that, in living animals, the motion of the heart is often greatly increased by a degree of acrimony in the blood imperceptible to the senses, and by the chyle which is much less acrid than those aliments from which it is prepared, and which the stomach and intestines bear without any pain, or remarkable increase of their peristaltic motion.

BUT if the irritability of the heart neither depends on its sensibility nor on its nerves, for what purpose has nature bestowed any upon it ? Since its motion is only of the involuntary kind, one would be apt to think it might have been better without nerves altogether; for, upon *M. de Haller's* system

† *Act. Gotting.* vol. I.

system of irritability, they seem to be of little other use †, than to make the heart liable, when diseased, to a painful sensation, from which it must have been exempted; had it been destitute of nerves.

## V. I†

† Vid. *Seconde lettre de Mr Caldani. Memoires sur les parties sensibles, tom. 3. &c. p. 371.*

Mr Caldani affirms, that the irritation of the *par vagum* and intercostals, after opening the *thorax*, neither accelerated nor renewed the contraction of the heart in lambs. But this authority will be more than balanced by that of *Willis* and *Lower*, *Kauu Boerhaave* and *Laghi*. *Kauu Boerhaave* tells us, that, after the heart's motion had become languid in dogs, whose *cranium* he had opened, he always observed, that it contracted more briskly when the *cerebellum* or *medulla oblongata* was irritated. *Laghi* says the same thing happened when he stimulated the cardiac nerves. And *Willis* and *Lower* have long since informed us, that when the *par vagum* and intercostals were tied, the heart was affected with uncommon tremors and palpitations. Indeed, when numerous experiments shew that the other muscles are convulsed by irritating their nerves, it must appear highly improbable, that the nerves of the heart have no power over it. However, we shall afterwards see, from the effects of *opium*, that the irritability of the heart depends upon the nervous power, as well as that of the other muscles.



V. It is said that irritability cannot depend on sensibility, because the greatest pains do not always occasion convulsive motions, and violent convulsions are often without pain. With the same view we are told that caustic vapours destroy the irritability of the heart without exciting pain, and that paralytic members retain their feeling after their power of motion is lost. See *Memoires sur les parties sensibles et irritables*, tom. iv. p. 92. & 93.

IN answer to these objections, it may be sufficient to observe, that,

(a) ALTHOUGH in the gout, rheumatism, toothach, and other diseases, there is often violent pain without any convulsions, because the parts affected are not muscular, or perhaps have no particular sympathy with any of the muscles, it can never be concluded, contrary to the evidence of numberless experiments and observations, that a violent pain in the muscles or their nerves does not produce convulsions. Besides the experience of every practical physician will attest, that acute pain is most apt to occasion

sion convulsive motions in those patients whose nerves are endowed with the most delicate feeling.

(*b*) In women, convulsions are often owing to sudden and strong affections of the mind, or to an uneasy sensation in the stomach or intestines. But as these causes produce convulsive motions by disagreeably affecting the nervous system, nothing can be concluded from them against the connection between irritability and sensibility; on the contrary, they are rather a proof, that the former of these powers depends on the latter.

FURTHER, as those general convulsions, which often happen to hysteric patients from a disordered state of the alimentary canal, can only be accounted for from the sympathy that obtains in the nervous system, they cannot be independent of feeling, without which there could, strictly speaking, be no consent of parts which are distant from each other.

THE whole body is often more disordered from a disagreeable sensation in certain nerves

nerves or organs, than from acute pain: Thus a *nausea* or sickness at stomach, particular smells and sounds, will affect some very delicate women with fainting and convulsions; and more violent motions are occasioned by tickling the soles of the feet, or the sides, than by cutting them with a sharp instrument: But from these examples it can never be justly inferred, that there is no connection between irritability and sensibility.

(c) Not only caustic vapours, but foul air, the steams of sulphur and of strongly fermenting liquors, quickly destroy animals, and put a stop to the motion of the heart; but, in the present argument, no conclusion can be drawn from this effect of those substances. *M. Caldani*, who is quoted by *M. de Haller*, argues, that irritability cannot depend on sensibility, because water or air makes the heart contract more briskly than caustic vapours. As a proof of this, he tells us, that when frogs were inclosed in a glass-receiver full of these vapours, the heart soon beat feebly, or not at all, and could not be made to move briskly by any irritation: He adds  
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that, since these caustic vapours applied to sensible parts occasion pain, it follows, that irritability is destroyed by those things which excite sensibility \*. Such indefinite experiments and ill-founded conclusions scarce deserve any answer.

CAUSTIC vapours, whether they excite pain or not, destroy the motion of the heart by their corrosive acrimony or poisonous quality; and the heart of a frog, immersed in boiling water or oil of vitriol, ceases immediately to move, because its nerves and fibres are greatly injured by them. Although, therefore, moderate *stimuli* excite and increase the motion of the heart, very hot, acrid and corrosive substances render it soon callous and incapable either of feeling or motion.

(d) WHEN a palsied leg or arm retains its sensibility, we can only conclude that voluntary motion may be lost, while the sense of feeling remains: For the muscles of members that are not only paralytic, but even withered  
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\* See *memoires sur les parties sensibles*, &c. tom. iii.  
p. 369. & 370.



in a good degree, may be excited into convulsive motions by *stimuli*, and therefore are not wholly deprived of their irritability. Of this I had a strong instance sometime ago in a man, whose left arm had been not only palsied for twelve years, but was much extenuated, notwithstanding which, the muscles of this arm were brought into contraction every time they received the electrical shock. But further, it ought to be observed, that, in paralytic cases, the nerves which go to the muscles may be, often, more obstructed or weakened, than those which are bestowed on the skin.

VI. As *opium* is well known to weaken or destroy the power of feeling in animals, I thought it might throw no small light on the controversy concerning irritability, to determine by experiments, how far that substance also weakened or destroyed the moving power of the muscles.

These experiments, which are to be found in my essay on the vital motions of

animals\*, and in the Edinburgh Physical Essays†, were made with care, and are related with precision ; nor have I ever trusted any one fact to a single experiment.

WHEN one would determine accurately the effects of *opium* on the motion of the heart, it is not sufficient to inform us, as some have done, that, after giving *opium* to dogs or frogs, the animals were opened, and their heart was observed to continue its motion, altho' the intestines had lost theirs. Experiments made and narrated in this loose uncircumstantiated manner are by no means decisive. The method I followed, was to open and inspect the animals, which were exposed to the action of *opium*, at different times, and to observe accurately the steps by which it first weakened, and at last put a stop to the motion of the heart : and in order to ascertain exactly the number of its pulsations, I always used a watch with a hand that marked seconds.

FROM

\* Pag. 370.—376.

† Vol. 2. art. xx. p. 280.—316.

FROM about twenty experiments conducted in this manner, I have clearly, and, as I have been told, to the satisfaction of several able Physicians, proved, that *opium*, injected into the stomach and intestines, or applied to the bare abdominal muscles of frogs, soon lessens, and, after some time, entirely destroys all feeling and power of motion, not only in the parts to which it is applied, but through the whole body. That *opium* produces these effects by its action on the ends of the nerves which it touches\*. That a solution of *opium* in water, conveyed into the stomach and intestines of frogs, soon renders the motion of the heart remarkably slow, and at length puts a stop to it altogether†. That *opium* weakens and destroys the motion of the heart, in frogs, much sooner than the destruction of the brain and spinal marrow.— That it operates much more slowly in destroying the

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heart's

\* See answer to object. 9. below.

† In half an hour it reduced the pulsations of the the heart in one frog, from above 60 to 17 in a minute:

heart's motion in frogs deprived of their brain and spinal marrow, than it does when these animals are entire; and that when applied to the bared abdominal muscles of the former, it seems to put a stop to the motion of the heart only a very little sooner, than would happen from the loss of the brain and spinal marrow alone †. Whence it follows, that *opium* weakens or destroys the moving power of the heart chiefly, if not wholly, by the mediation of the brain and spinal marrow, and consequently that the motion of the heart depends

nute: And in another frog, after an hour and six minutes, the heart did not move above seven times in a minute. See Edinburgh Physical Essays, vol. 2. p. 281. & 282.

† The heart of a frog, thirty-five minutes after it was deprived of its brain and spinal marrow, beat thirty times in a minute. The heart of another frog, whose brain and spinal marrow were destroyed, beat 26 times in a minute, after a solution of *opium* in water had been applied, thirty-six minutes, to its bared abdominal muscles: But after a solution of *opium* had been applied in the same manner, and for the same time to an entire frog, its heart only beat six times in a minute. *Vid.* Edinburgh Physical Essays, vol. 2. art. xx. no. 5. 6. and 7.



pend upon the influence of the nerves, which it receives from those parts. The destruction of the brain and spinal marrow only prevents the derivation of any new *influence* from these parts to the heart, but does not immediately destroy the power of the cardiac nerves themselves. On the other hand, *opium*, applied in sufficient quantity to the sensible parts of frogs, not only soon puts a stop to the action of the brain and spinal marrow, and thus produces the same effect upon the heart as does the loss of those parts, but also destroys the power of every nervous filament proceeding from them, and therefore puts a stop to the motion of the heart in frogs, sooner than the destruction of the brain and spinal marrow.

I have shewn likewise, from the observations of Dr *Kaau Boerhaave*, \* and from an experiment made at my desire by Dr *Ramsay*, that opium, introduced into the stomach, or injected into the cavity of the *abdomen*, soon renders the pulse remarkably slower in dogs: And it is well known that men who

\* *Impet. faciens Hippocrat. dict. §. 434. 435. & 436.*

who have swallowed *landanum* by mistake, in so great quantity as to occasion death, have had a slower pulse and respiration, than patients who die either of common diseases, or by other poisons.

To my experiments made on animals with *opium*, and the conclusions drawn from them, *M. de Haller* has made the following objections, which I have been at pains to collect and range in some kind of order, subjoining to each, what, it is hoped, will be thought a sufficient answer.

*Objection 1.* *Opium* heats the body, produces sweat, and quickens the pulse, instead of making it slower, as *Dr Whytt* has alleged \*.

*Answer.* ALTHO' *Opium* in a moderate dose, generally heats the body a little, promotes sweat, occasions thirst, and renders the pulse somewhat fuller; yet I am not so certain of its effect in accelerating the motion of the heart. But supposing *opium* in a small quantity always quickened the pulse,  
it

\* *Mém. sur les parties sensibles, &c. tom. iv. p. 123.*

it would not thence follow, contrary to the clearest experiments on brutes, that it must have the same effect when taken so liberally as to endanger life ; for we know, that notwithstanding it gives spirits and even vigour to many people who are accustomed to it, yet if used, even by them, in excess, it produces a stupor and a paralytic debility for some time \*. *Opium*, in a moderate dose, may, by its stimulating quality, heat the body and quicken the pulse a little; while a greater quantity of it may so weaken the sensibility and active power of the whole nervous system, as, notwithstanding its *stimulus*, to render the motion of the heart more languid. Agreeably to this, Dr *Langrish* has observed, that an ounce of laurel-water given to a dog occasioned much stronger convulsions, than five or six ounces. The first quantity was sufficient to irritate the nervous system, the last quickly destroyed the powers of life altogether, and therefore prevented the laurel-water

\* *Kaui* Boerhaave. Impet. faciens Hippocrat. dict. § 438.

water from producing, by its *stimulus*, any considerable convulsive motions\*.

BUT, whatever may be the effect of *opium*, in a small dose, upon men, nothing is more certain, than that it renders the pulse remarkably slower in frogs and other animals†. My very ingenious colleague Dr *Alexander Munro jun.* who has been lately employed in making many curious experiments with *opium* on frogs, laid bare the heart of one of these animals, and then poured thirty drops of a filtered solution of *opium* in water into the cavity of the *abdomen*. In two or three minutes after this, the heart did not beat above half its usual number of times in a minute: And in four minutes its pulsations were reduced almost to one third of their usual number. In five or six minutes the blood ceased to move in the small vessels of the foot; altho', upwards of four hours after the solution

\* See Langrish's Physical Experiments on brutes, p. 67.

† Vid. note, p. 281, & 282. above.



tion of *opium* was dropped into the cavity of the *abdomen*, the heart was observed to perform about a dozen of very feeble contractions.

In a frog, which I immersed in a solution of *opium*, after laying open its whole *abdomen* and *thorax*, the motion of the heart was, in eleven minutes, reduced to less than one fifth of its usual quickness, and in about twenty minutes it ceased altogether.\*

*Object. 2.* The diminution of the quickness of the pulse in that dog, into whose *abdomen* Dr *Ramsay* injected a solution of *opium*, was owing to the opening of the *thorax*, and drawing asunder the ribs: And Dr *Wytt* certainly exaggerates matters greatly, when he represents this dog's pulse as beating 150 times in a minute, because this is too great a number to be counted †.

*Answer.* M. de Haller has forgot, that, before either the ribs were hurt, or the *thorax* laid open, the pulse of this dog was reduced

\* Edinburgh Phys. Essays, vol. 2. art. xx. no. 11.

† *Memoires sur les part. sensib. &c.* tom. iv. p. 126.

ced from 150 to 76 in a minute\*. The effect of the *opium* was so sudden in this experiment, that four minutes after it was injected into the *abdomen*, the dog was rendered so insensible, that he felt no pain when the teguments of his *thorax* were dissected.

WITH regard to the number of this dog's pulse before the injection of the *opium*, I know Dr *Ramsay*'s accuracy too well to suspect that he could be so far mistaken, as any ways to affect the conclusion to be drawn from this experiment: And if Dr *Langrish* observed the pulse to beat 84 times in a minute, in a dog 22 inches high, † it cannot appear altogether improbable that, in Dr *Ramsay*'s dog, who was not seven inches high, and only six months old, the velocity of the pulse might be 150;‡ for  
altho'

\* See Edinburgh Physical Essays, vol. 2. p. 298. and 299.

† Physical experiments on brutes, p. 95.

‡ It may be proper to observe, that this dog, having had several experiments made on him before, seemed to be greatly frightened, when Dr *Ramsay* laid on him; and that on this account his pulse might, perhaps, beat

altho' the heart of a full grown man does not commonly contract above 70 times in a minute, yet in a child of half a year old, it makes nearly 130 vibrations in that time.

WHEN *M. de Haller* says a pulse of 150 in a minute is too quick to be numbered,

B b he

beat twentytimes or more in a minute, than if the animal had been quite free from fear. This circumstance however, which I was only lately informed of by *Dr Ramsay*, cannot invalidate the conclusion which I have drawn from his experiment, because this dog's pulse, four minutes after the solution of *opium* was injected into the *abdomen*, only beat 76 times in a minute.

But in order to remove all reasonable doubt concerning this matter, *Dr Ramsay* has taken the trouble to make the following experiment, which I shall relate, in his own words.

— "The experiment I made this afternoon to determine the quickness of the pulse in a small dog, was as follows.

"The dog was seven weeks old, and weighed exactly twenty ounces; his pulse beat from 15 to 18 in five seconds, which, at the lowest, makes 180 pulsations in a minute; and that I might be under no mistake,

I

he shews himself not to have attended with any accuracy to the velocity of the pulse in several diseases. Thus in the fever preceeding the eruption of the small-pox, in children from two to four years, I have most commonly observed the pulse to beat 14 or 15 times in five seconds *i. e.* from 166 to 180 in a minute. In a peripneumonic fever in children of two or three years, I have met with a pulse which beat 16 or 17 times in five seconds. In such patients as died of a dropsy in the ventricles of the brain I have generally, a day or two before their death, observed the pulse

“ I examined both the pulsation of his heart through  
 “ the ribs and the beating of the arteries of his legs.  
 “ I continued the examination a quarter of an hour,  
 “ during which, I felt the pulsations both of the heart  
 “ and arteries about twenty times. As I gave the dog  
 “ no pain, and as he seemed fond of me, I conclude  
 “ the above to be his natural pulse; and if I can be  
 “ certain of any thing, I have made no mistake in this  
 “ last experiment. It is obvious to observe that the  
 “ quicker motion of the heart in this dog, than in that  
 “ one into whose *abdomen* I afterwards injected a so-  
 “ lution of *opium*, must have been owing to his being  
 “ much younger and of a smaller size.



pulse to beat from 150 to near 200 ; and a physician of this place assured me, that in a patient of his who died of this disease, the pulse moved at the rate of 216 times in a minute.

*Object. 3.* *M. de Haller* could never kill a dog with *opium*, in whatever dose he gave it ; and therefore this substance does not destroy the moving power or irritability of the heart. \*

*Answer.* It is certain that Dr Mead † and others have killed dogs with *opium*, and practical physicians have had too frequent experience of its proving fatal to the human species, when taken in an immoderate dose ‡ : Frogs and many other animals are likewise destroyed by it. But supposing that *opium*, even in large quantities, did

B b 2.

not

\* *Mem. sur les parties sensibles, &c. tom. iv. p. 126.*

† *Treatise on poisons, Essay iv.*

‡ *Histoire acad. Royale des sciences, 1735, p. 6. Essay on the vital motions, p. 194. and Edinburgh Med. Essays, vol. 5. part. 1. art. xii. sect. iii.*

not kill dogs, yet, if in a few minutes it renders their pulse remarkably slower than usual, it cannot be denied that it lessens the irritability of their heart, while it destroys, at least for a time, the sensibility and moving power of all the other parts of the body.

*Object. 4.* Since *opium* which destroys feeling, does not put a stop to the motion of the heart, the irritability of this organ cannot depend on its sensibility \*.

*Answer.* I have shewn from various experiments † which are confirmed by those of *Kaau Boerhaave* ‡ and *Alston* || that *opium* soon renders the motion of the heart remarkably slow in dogs and frogs, and at last puts a final stop to it. Whence it appears that *opium*, which lessens or destroys the power of feeling in animals, also lessens or destroys the motion of the heart, as well as

\* *Mémoires sur les parties sensibles*, tom. iv. &c. p. 128.

† *Edinburgh Physical Essays*, vol. 2. art. xx.

‡ *Impet. faciens Hippocrat. dict.* §. 434, 435, & 436.

|| *Edinburgh Medical Essays*, vol. v. part. 2. art. xii.

as that of the other organs. *M. de Haller's* experiments, from which he has concluded that *opium* does not affect the moving power of the heart, really prove no such thing; they only inform us that three frogs were opened after having swallowed *opium*, and that the heart continued to move, after the motion of the intestines had ceased †. But as it is not said, how long the *opium* had remained in the stomach of these animals before they were opened, nor what proportion the heart's motion bore to its usual quickness, we can draw no certain conclusion from these experiments, except that *opium* received into the stomach of frogs, destroys the motion of the intestines sooner than that of the heart. In my experiments, altho' *opium* forced into the stomach and intestines of frogs, intirely destroyed

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the

† *Memoires sur la nature irritable*, tom. i. sup. 528, 529, 531. Since *M. de Haller* has added, that when the motion of the heart failed, it was renewed by stimuli, it is not to be doubted that the *opium* had affected it considerably; because the heart of a frog, which has got no *opium*, continues to beat for many hours, after its thorax is laid open.

the power of the muscles of voluntary motion in about half an hour, yet the heart continued to move a considerable time after this, but with great slowness and seeming languor\*. This organ, therefore is not exempted from the power of *opium*; altho' it is not so soon affected by it, as the other muscles.

SINCE it is acknowledged on all hands, that *opium* destroys the irritability of the intestines and other muscles, as well as their sensibility; its affecting the heart more slowly must be owing, either to the *opium* not being applied so near to it†, or to this organ

\* See Essay on the vital motions, p. 371 & 372; and Edinb. Physical Essays, vol. 2. p. 281 & 282.

† When a frog, whose *thorax* and *abdomen* were opened, was laid in a solution of *opium* in water, the voluntary muscles were deprived of their irritability in ten or twelve minutes, and the heart's motion ceased in twenty minutes. Whereas, when the solution of *opium* was injected into the stomach and intestines of another frog, its heart continued after an hour and seven minutes to move at the rate of seven times in



organ-being endowed with a stronger power of motion ; for it is by no means probable, that the irritability of the heart depends on a cause quite different from that of all the other muscles.

Object.

in a minute, *i. e.* about eight or nine times slower than it does in a natural state. *Vid.* Edinb. Phys. Essays, vol. 2. p. 282 & 291.

But that *opium* when applied to the heart itself, destroys its power of motion as soon as that of the other muscles, appears from the following experiment of Dr *Al. Monro junior*. The Doctor having laid bare the heart, and then injected into a large vein, which runs along the under and middle part of the *abdomen* on the outer side of the *peritonæum* in frogs, a few drops of a solution of *opium* in water, *viz.* twelve drops into two frogs, and six drops into a third; he observed, that as soon as the *solution* had reached the heart, that organ was rendered incapable of expelling its contents, and, in less than one minute after this, became so intirely paralytic, as not to make the least contraction, on the strongest irritation, whether applied to its outer or inner surface.

This experiment of Dr *Monro*, while it confirms what I had advanced concerning the effect of *opium* in destroying the moving power of the heart, is intirely subversive both of *M. de Haller's* experiments, and of the conclusions he has drawn from them.

*Object.* 5. Dr *Whytt*, by shewing that the motion of the heart in frogs continues after the destruction of the brain and spinal marrow, has proved against himself that the irritability of the heart is equally independent of the nerves and of sensibility.\*

*Answer.* The motions produced by irritating, the spinal marrow shew that it retains its power over the muscles after the loss of the brain: And the convulsions produced by irritating the crural nerve of a frog after it is separated from the body, prove that the nerves continue to preserve their power of putting the muscles in motion, after their communication with the brain and spinal marrow is quite cut off. Wherefore the continuance of the motion of the heart in frogs, is no proof that the moving power of this muscle does not depend on its nerves, and consequently on its sensibility †.

FURTHER that the motion of the heart is not independent on the brain and spinal  
MARROW

\* *Memoires sur les parties sensibles, &c. tom. iv. p. 130.*

† See above p. 245—263. where this point is more fully discussed.

marrow evidently appears from some experiments lately made by Dr *Al. Monro junr.* who has observed that as soon as these parts are destroyed, the moving power of the heart becomes so weak, that it is unable to carry on the circulation in the vessels of the hind-legs ; notwithstanding that, immediately after the destruction of the brain and spinal marrow, the motion of the heart becomes, for a little time, quicker than usual.

It is observable, that as those animals whose blood is almost as cold as the *medium* surrounding them, live a considerable time after the loss of their heart, so their nerves preserve their powers much longer, after the destruction of the brain and spinal marrow, than the nerves in men and other animals, whose life ceases when the circulation of the blood is stopt, and whose nerves soon lose all their powers, after they are separated from their origin.

*Object. 6.* When *opium* is given in an immoderate dose, so as to kill animals, it must like other poisons weaken all the powers of

of life, and therefore must render the pulse slower before death \*.

*Answer.* Although every poison and every disease, which prove mortal, destroy the moving power of the heart at last; yet most poisons and diseases render the pulse quicker in men, than it is naturally, till just about the time of death, or a very little before it, when the patient becomes in a great measure insensible, and all the powers of life begin to cease; and *M. de Haller* himself has remarked that poisons almost always increase the peristaltic motion of the intestines in animals †. A horse who was killed in three days by repeated doses of laurel-water, a little before his death, had a pulse which beat at the rate of 103 in a minute, *i. e.* three times quicker than it had done, when the animal was in health ‡. A dog 22 inches high, by swallowing, for above four months, the

\* *Memoires sur les parties sensibles*, tom. iv. p. 128 and 129.

† *Memoires sur la nature irritable*, tom. i. p. 339.

‡ *Langrish's Physical Experiments*, p. 72.



the powder of green laurel leaves, had his pulse, before he died, raised from 84 its natural quickness to 157 strokes in a minute†. *Opium*, on the contrary, given in a great dose, soon renders the pulse much slower in dogs and frogs, and this slowness gradually increases till the motion of the heart ceases altogether.

BUT the following experiments which I made lately, will demonstrate beyond doubt that *opium* renders the motion of the heart slower than other poisons.

(a) HAVING laid open the *thorax* and *abdomen* of a frog, I poured into the *abdomen* a tea spoonful of water in which I had dissolved two grains of *opium*. In a minute and a half, the pulsations of this animal's heart were reduced from 54 to 26 in a minute. After five minutes the heart beat only 18, and after twenty minutes only ten times in a minute.

AN hour after the solution of *opium* was applied to the *viscera* of the *abdomen*, the heart

† *Langrish's Physical Experiments*, p. 95 & 103.

heart made only between six and seven very feeble pulsations in a minute, and in less than 15 minutes after this its motion ceased altogether.

(b) HAVING laid open another frog, I poured into its *abdomen* a tea spoonful of water, in which I had dissolved three grains of corrosive mercury. In five minutes, the pulsations of the heart were reduced from 54 to 46 in a minute. After ten minutes the heart beat 38, and after twenty minutes 18 times in a minute, but its contractions were extremely feeble.

In less than half an hour from the application of the solution of the corrosive mercury, the motion of the heart ceased, and the animal seemed to be quite dead.

(c) I laid open a third frog, and poured into its *abdomen* a tea spoonful of malt spirits nearly of the same strength with common French brandy. In a minute and a half, the pulsations of the heart were reduced  
from

from 54 to 52 in a minute\*. After five minutes the heart beat 50, and after eleven minutes 41 times in a minute. Fifty minutes after the malt spirits were applied to the *viscera* of the *abdomen*, the auricle and heart made 34 very feeble contractions, in a minute. In ten minutes more, the animal was become altogether insensible, and made no motion when its muscles were pricked or torn; but the auricle continued to beat 33 times in a minute, and the heart made nearly the same number of extremely feeble and incomplete contractions.

It was observable, that the heart of this frog was much paler than that of the frog to which the *opium* was applied, nor was

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there

\* As I had formerly observed the hearts of frogs to beat about 60 times in a minute, when their *thorax* was opened, I suspect its slower motion in the above three frogs might be owing to their having been kept in the house five days before making these experiments: Perhaps also the hearts of frogs beat slower towards the end of August than in June or July, when my former experiments were mostly made.

there any blood collected in its auricle. See *Essay on the vital motions*, p. 371 and 372; also *Edinburgh Physical Essays*, vol. ii. p. 282, 286, 290 and 313.

WHETHER an apoplexy occasioned by spirit of wine kills animals without quickening their pulse before death, as *M. de Haller* seems to insinuate\*, I shall not say; but I know certainly, that in other apoplexies the pulse, though at first slow, becomes, almost always, considerably quick before the patient's death. I have seen it rise from 60 or 65 to above 150 beats in a minute.

I have observed the heart of a frog moving 20 times in a minute, near two hours after the destruction of its brain and spinal marrow; whereas, in little more than an hour after injecting *opium* into the stomach and intestines of another frog, the heart beat only seven times in a minute†. When *opium* is given in such quantity to frogs

as

\* *Memoires sur les parties sensibles*, tom. iv. p. 129.

† *Edinburgh Physical Essays*, vol. ii. 282, 283, & 284.



as not to kill them, their pulse is rendered by it remarkably slow for many hours; but afterwards, when the effects of this poison begin to abate, the pulse gradually returns to its natural quickness, the intestines recover their peristaltic motion\*, and the other parts of the body regain their sensibility and moving power.

UPON the whole, we may conclude, that the remarkably slow pulse of those animals which are killed by *opium*, is owing to the peculiar virtue which this substance has of weakening or destroying the moving power of the heart, as well as the sensibility and irritability of all the other parts of the body.

*Obsect. 7.* *Opium* may, perhaps, by its viscosity alone, put a stop to the motion of the heart mechanically †.

C c 2

*Answer,*

\* *Vid. Memoires sur la nature irritable, tom. 1. exp. 456.*

† *Memoires sur les parties sensibles, &c. tom. iv. p. 129, & 130.*

*Answer.* That a scruple of *opium*, which has been known to kill a full grown woman \*, should render the whole mass of blood so viscid, as to put a stop mechanically to the motion of the heart, is a supposition that needs not be refuted. What becomes of this mortal viscosity of *opium*, when it is swallowed daily to the quantity of two or three, nay even ten drachms, by those who have been long accustomed to it? Although the nerves, by long use may suffer less from the action of *opium*, yet no custom could prevent the blood from being rendered proportionally more viscid by it.

*Object. 8.* To assert that *opium* kills frogs, more quickly when they are intire, than when they are deprived of the brain and spinal marrow, is a paradox, which would scarcely be rendered probable by the greatest number of experiments †.

*Answer*

\* Edinburgh Medical Essays, vol. v. art. xii. sect. iii.

† *Memoires sur les parties sensibles, &c.* tom. iv. p. 129.

*Answer.* Many things seemingly improbable have been proved to be true. Those experiments which I have related, were made with care, and no one has yet pretended to say that he has repeated them, and found the event different. But if *M. de Haller* had allowed himself to consider this matter more coolly, he would have seen no inexplicable paradox here; for if *opium* produces its effects by acting on the extremities of the nerves which it touches, and if the nerves have no communication or sympathy, but through the mediation of the brain and spinal marrow, it must necessarily follow, that *opium* applied to the abdominal muscles of a frog deprived of the brain and spinal marrow, can have little or no influence on its heart †; and therefore,

C c 3

will

† In this case the *opium* can, in no other way, affect the heart or its nerves, except in so far as some of its finer parts are absorbed by the bibulous veins of the abdominal muscles, and carried along with the blood to that organ: (a) And as Dr *Al. Monro junior*

has

(a) See Edinburgh Physical Essays, vol. 2. p. 303, and 304.

will not kill it so soon as if the animal had been entire.

*Object. 9.* Dr *Whytt's* experiments with *opium* on frogs, have been proved to be *fundamentally* erroneous by *Fontana*, who has shewn that *opium* applied externally, and its solution in water applied to the nerves, does not, in any degree, destroy that power whereby they make even the muscles of voluntary motion contract \*.

*Answer.* *M. Fontana's* experiments are not more contradictory to mine, than to those of *M. de Haller*, which shew that *o-*  
*pium*

has found by experiments that the moving power of the heart and action of the vessels is so weakened, in frogs, by the destruction of the brain and spinal marrow, that the circulation in many of the small vessels either ceases altogether, or goes on very languidly, it must follow, that the finer parts of the *opium* will be very slowly absorbed, and consequently can have but little influence in destroying that power of motion, which remains in the heart after the brain and spinal marrow are destroyed.

\* *Memoires sur les parties sensibles, &c. tom. iv.*  
p. 130, & 131.



*opium* destroys the sensibility of the nerves, and the irritability of all the muscles except the heart\*. The truth is, that when they are fairly represented, they are not directly repugnant to either. *M. Fontana* does not say, that *opium*, applied externally to frogs, has no effect in destroying the power of the nerves†. He only tells us, that a solution of *opium* in water, applied to the bared trunk of the crural nerves of frogs did not, in any degree, destroy their power;

\* *Vid. Memoires sur les parties sensibles, tom. 1. p. 237, & 339, Opium supprime cette faculté des nerfs par laquelle ils excitent du mouvement dans les muscles.*

† There is nothing more certain than that a solution of *opium* in water, applied externally, kills frogs; I shall therefore content myself with mentioning the following experiment, which is altogether decisive of this matter. Dr *Al. Monro junior.* having applied to the belly and hind legs of a frog some lint moistened with about a hundred drops of a solution of *opium* in water; in less than three hours, the animal was convulsed and unable to move its body out of the place where it lay, and after five hours was quite dead; for although the lint was removed and the skin washed, it did not recover.

power\*; but he does not alledge that *opium* applied to the stomach, intestines or other parts, where the nerves terminate, has no such effect. *M. Fontana's* experiments, therefore, prove nothing, except that *opium* does not destroy the power of the nerves, when it is applied to their trunks, in the the same manner that it does, when it touches their extremities; a fact that will scarcely be doubted of by any one, who considers, that in the former case, the medullary substance of the nerves is defended by their coats which are in a great measure insensible†.

ALTHOUGH *M. Fontana's* experiments are neither of that nature, nor related with

\* *Memoires sur les parties sensibles, &c. tom. iii. p. 211.*

† *M. Fontana*, found that although spirit of wine destroyed the power of the nerves in that part which it touched, yet, when these nerves were irritated a little below this, the muscles contracted as usual (a). The same thing is true of a ligature and of a red hot iron, oil of vitriol or other acrid substances, whose action is totally different from that of *opium*, which affects not only the nerves it touches, but the whole system.

(a) *Mem oires sur les parties sensibles, tom. iii. p. 466.*

with that precision, which would be necessary to determine the operation of *opium*, yet he has inferred from them, that *opium* does not produce its effects on animals, except it be introduced into the blood; and that, when it is carried to the muscles, it changes the nature of their fibres, or of their glue, which, he thinks, is probably the seat of irritability. This conclusion is not only contrary to what the best writers have taught concerning the action of *opium*\*, but is utterly inconsistent with several experiments, that are altogether decisive as to this matter, and which *M. Fontana* seems to have been unacquainted with. Thus the paralytic weakness brought

\* See *Jones's* mysteries of *opium*. *Mead's* treatise on poisons. *Alston* in the *Edinburgh Medical Essays*, vol. v. part. i. article xii. *Kaan Boerhaave imper. faciens Hippocrat. dict.* § 434—438. And the Baron *Van Swieten's* comment. in *Boerhaav.* aphor. 229. No. 2. More authors might be mentioned, but these are fully sufficient to balance the authority of *Tralles*, who, though greatly extolled by *M. de Haller*, seems to have erred not a little in his account of the action of *opium* upon animals.

brought on a dog's hinder legs, in a minute after injecting a solution of *opium* into his intestines, and almost instantaneously after it was thrown into the cavity of the *abdomen*, cannot possibly be owing to the finer parts of the *opium* entering the blood, and changing the nature of the muscular glue. Further, frogs, which live above two hours and a half after being deprived of their heart, are killed in half an hour after the loss of that organ, by injecting a solution of *opium* in water into their stomach and intestines \* ; although in this case, the *opium* could neither enter the blood nor be carried by it to the muscles ; while, on the other hand, *opium* has very little effect in hastening the death of those frogs, which have been deprived of their brain and spinal marrow.

As far as we can judge from experiments, *opium* acts either on the extremities of the nerves to which it is first applied, in which way it seems to produce its speediest effects, or by being carried, by the absorbent veins, into  
the

\* Edinburgh Physical Essays, vol. ii. p. 281, and Essay on the vital motions, p. 37.



the blood, where it affects the nerves of the whole vascular system. But as the effects of *opium* generally cease, in a great measure, in men, by the time it may be supposed to have got all out of the stomach and intestines, and to have made its way into the blood, we may conclude, that the chief action of this substance in *them*, is on the nerves of the stomach and intestines, to which it is first applied.

VII. I am here, naturally led to consider *M. de Haller's* notion of irritability as an active property of the glutinous matter which partly composes the muscles; but as he has advanced nothing new in support of this doctrine\*, I shall only observe, that since it has been proved that the moving power of the muscles depends upon their nerves, we are under no necessity of ascribing this power, either to their glutinous, or earthy part. The tendons and ligaments which abound much more in glue, than the  
muscles

\* *Vid. Memoires sur les parties sensibles, &c. tom. iv. p. 123 & 124.*

muscles, are not irritable; and the greater irritability of young animals, is doubtless owing to their greater sensibility, which is also the cause of their being so remarkably affected by *opium*. Women of the most delicate frame have often thinner blood and less, or at least not more glue in their solid parts, than strong men, whose muscles are not liable to be affected with convulsions from slight causes; and it is demonstrable, that those convulsive motions which proceed from the irritation of a distant part, are owing solely to the sympathy of the nerves.

Strange, certainly it is, to find men of learning and abilities, misled by a few ill understood experiments and trusting to metaphysical notions, using every art to prove, that the irritable power of the muscles does not depend on their nerves, and ascribing it to a substance, the most unlikely to be possessed of it; a substance which, devoid of active powers, appears remarkably inert, and is not endowed even with elasticity, in so remarkable

able a degree, as glass, fine wool, hair, and many other substances\*.

AND thus I have endeavoured to shew that *M. de Haller* has equally failed in the support of his own theory, and in the arguments he has brought to confute what I had advanced concerning the nature of irritability. Nor can I help thinking it a strong presumption against my learned adversary's opinion, that so able a writer has argued so inconclusively in its defence; for to express myself in the words of the Poet,

— *Si Pergama dextrâ  
Defendi possent, etiam hæc defensa fuissent.*

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\* When *M. de Haller* says that even the glue of animals separated from them, shews as it were the remains of irritability, because it contracts itself after being drawn out †, he mistakes elasticity for the remains of irritability; and has forgot that the glue got by boiling the ligaments and skin is stronger, and possessed of this elastic power in as great, or a greater degree, than that which the muscles afford.

† *Memoires sur les parties sensibles, &c. tom. iv. p. 124.*

I shall only add, that if there should be found a few *phenomena* which, at first sight, do not appear so favourable to the doctrine of irritability as depending upon the nerves, and being connected with sensibility, this would be far from being a sufficient reason, for rejecting an opinion, which is supported by such a chain of argument, and agrees so well with the various *phenomena* of the animal frame. There are few even of the best founded theories in natural philosophy or physic, which are not liable to difficulties and objections, that, sometimes, cannot be easily removed. At present, the greatest philosophers have only access, as it were, to the surface of things, without being able to penetrate into many of the mysteries of nature.

F I N I S.



## E R R A T A.

Page 111. line 5. *for at read of.*

P. 112. l. 14. *for in r. under*

P. 123. l. 1. *for would r. should*

P. 128. l. ult. *for vol. 11. r. vol. I.*

P. 130. l. 21. *for are r. is.*

P. 149. l. 8. *for excited r. excite.*

P. 197. l. 22. *after medicinæ add vol. 12.*

P. 243. l. ult. *for 253 r. 223.*

P. 245. l. 17. *for 1. r. I.*

P. 250. l. 26. *for produirent r. produeront;*

P. 251. l. ult. *for opium r. destroy'd by.*

P. 252. l. 13. *for 2. r. II.*

P. 270. l. 8. *for cuticles. r. cuticle.*

P. 288. l. penult. *after laid add holds.*

THE R. R. A. T. A.

1. The first thing I saw when I
2. went to the office was
3. a man in a suit and tie
4. who was talking to a woman
5. who was sitting at a desk
6. and looking at some papers
7. and I was standing there
8. and looking at them
9. and I was looking at them
10. and I was looking at them
11. and I was looking at them
12. and I was looking at them
13. and I was looking at them
14. and I was looking at them
15. and I was looking at them
16. and I was looking at them
17. and I was looking at them
18. and I was looking at them
19. and I was looking at them
20. and I was looking at them

